Shaping the DREAM: Law as Policy Defining Undocumented Students' Educational Attainment

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Dedication

I dedicate this thesis to the ancestors who never enjoyed the freedoms promised them by the Constitution and the youth who live in the shadows in hope that our same American Dream will one day be their reality.

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Abstract

In this two-paper dissertation, I examine U.S. Census data from the Current Population Survey Merged Outgoing Rotating Groups to understand how undocumented-student high-school-diploma, college-enrollment, associate's-degree, and bachelor's-degree attainment odds have been impacted by the Illegal Immigrant Reform and Immigrant Responsibility Act of 1996 (IIRIRA), which restricted in-state-tuition subsidies to undocumented students, and by in-state-residency-tuition (ISRT) laws that states have passed beginning in 2001, to moderate the effect of IIRIRA. I use difference-in-difference estimation strategies to attempt to establish causal effects. Using Mexican foreign-born-non-citizen status as a proxy for undocumented status, and therefore the treatment group, I compare enrollment and degree-completion outcomes for college-aged likely undocumented persons before and after the laws' effective dates, treating the laws as an exogenous shock, with similarly situated documented persons as a control group. I find that IIRIRA led to sharp declines in educational attainment among likely undocumented youth, and that ISRT has been helpful, but alone insufficient to cure the harms caused by IIRIRA. Using a blended framework that uses liminal legality to understand the college choices of undocumented youth, I conclude, after Abrego and Gonzales (2010) and others that undocumented residency status as administered by IIRIRA converges over time to be a master status that makes the cost of attending college prohibitive while nearly eliminating any benefits. My research has implications for the continuing debate over the proposed Development, Relief, and Education for Alien Minors (DREAM) Act, the Obama administration's Deferred Action on Childhood Arrivals (DACA) programs, the education of undocumented students and the legal treatment and incorporation of undocumented people into U.S. society.

A DREAM Deferred: The Effects of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 on Secondary and Post-Secondary Educational Attainment among Undocumented Youth

Matthew Patrick Shaw

2016

I. Introduction

An estimated 11.3 million undocumented immigrants[1] live in the U.S., comprising 4 percent of the national population (Passel and Cohn 2009; 2015). Nearly 40 percent of undocumented Americans are 30 years old or younger; and 10 percent of undocumented immigrants are youth under the age of 18 (Gonzales 2011; Passel and Cohn 2015). Because of the U.S. Supreme Court decision, *Plyler v. Doe* (1982), undocumented youth have the same legal access to a free, public high-school education as their citizen peers. Yet, they complete high school at alarmingly lower rates. Only 60 percent of undocumented immigrants ages 18-24 have completed high school, compared to 85 percent of authorized immigrants[2] and 92 percent of U.S-born residents (Passel and Cohn 2009). Some of this differential is believed to be driven by the arrival of undocumented youth during their teenage years. These youth often come to the U.S. to work, and never enroll in high school. Among the annual cohort of approximately 80,000 undocumented youth under the age of 18 who have lived in the U.S. for at least five years, 15,000, or 20 percent, do not graduate from high school (Passel 2009).

Of the undocumented residents who do graduate from high school, even fewer go on to college relative to their authorized-immigrant and citizen peers. Only 25 percent of

[1] An undocumented immigrant is a non-U.S.-citizen or national who is "unlawfully present" in the U.S. Undocumented immigrants include persons who migrated to the U.S. without obtaining a "green card" or an immigrant visa and persons who obtained non-immigrant visas and overstayed the authorized time period. Undocumented minor children, though technically incapable of being "unlawfully present" in the U.S. (see 8 U.S.C. § 1182(a)(9)(B)(iii)(I)), cannot obtain authorizing papers (e.g., social security number) necessary to work or obtain certain licenses (8 U.S.C. § 1182).

[2] An authorized immigrant is a non-U.S.-citizen or national who has been granted by the United States Citizenship and Immigration Services (USCIS) or its predecessors the right to reside in the U.S. and work or go to school. Authorized immigrants include Lawful Permanent Residents who hold "green cards," holders of immigrant visas, which may have restrictions on where an authorized immigrant may work or attend school. Temporary migrants who have USCIS permission to reside in the U.S. while on business or cultural exchange, attending school, attached to a diplomatic detail, or otherwise working are not immigrants according to IIRIRA (8 U.S.C. § 1101 et seq).

undocumented residents ages 25-61 have attended college, and only 15 percent have completed a bachelor's degree (Passel and Cohn 2009). In comparison, 53 percent of authorized immigrants of the same age have attended college, and 35 percent have completed a bachelor's degree; and 62 percent of U.S.-born residents have attended college, with 32 percent completing a bachelor's degree (Passel and Cohn). Among Mexican, undocumented immigrants, who make up 59 percent of the total U.S. undocumented population, the disparities are starker. Only 36 percent of Mexican undocumented immigrants have completed high school; only 4 percent hold a bachelor's degree (Passel and Cohn).

These disparities are alarming because a college education is increasingly necessary for financial security and socioeconomic mobility (cf. Deming and Dynarski 2010; Gonzales 2009). In 2007, the median household income for undocumented residents was $36,000, 28 percent lower than the $50,000 median household income for all other residents (Passel and Cohn 2009), and at least part of this disparity can be explained by differences in education levels. As a result, 20 percent of undocumented adults—and 33 percent of their children—live below the poverty line (Passel and Cohn 2009), which in 2007 was $16,530 for the average U.S. household size of three people (U.S. Census 2007).

Gonzales (2011; 2016) attributes these differences in educational attainment to undocumented youth's needing to "learn[] to be illegal" as they approach adulthood. Most undocumented youth come to the U.S. in early childhood, before their age peers start high school. Commonly termed the "1.5 generation," these youth are culturally indistinct from their citizen peers, having grown up in the same schools, having been

exposed to the same culture, and having developed English-language fluency alongside them (Gonzales 2009). Because of *Plyler* and the Family Educational Rights and Privacy Act of 1974 (FERPA), undocumented youth are largely shielded from the day-to-day consequences of their residency status until they approach adulthood (Abrego and Gonzales 2010). This suspension of "illegality," [3] what Menjívar (2006) and Abrego and Gonzales (2010) term "liminal legality," ends at adulthood due to the activation of "unlawfully presence" provisions of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 ("IIRIRA," 110 Stat. 3009-546, codified at 8 U.S.C. § 1101, 1182 et seq.).

Many, but not all, of IIRIRA's provisions apply to individuals who are "unlawfully present" in the U.S. (see 8 U.S.C. § 1182). Minors are, by definition, incapable of being "unlawfully present" in the U.S. However, on his or her 18th birthday, he or she has less than 180 days to leave the U.S. or he or she will be "unlawfully present" in the U.S., and if expelled from the country, could not return for three years if "unlawfully present" for less than a year, or 10 years if "unlawfully present" for a year or more (8 U.S.C. § 1182(a)(9)(B)(iii)(I)). IIRIRA limits undocumented residents' ability to work and to receive licenses. Because IIRIRA also made it easy to deport residents who were found "unlawfully present," it discourages freedom of travel and encourages participation in a clandestine economy. Though IIRIRA imposes an obligation for "unlawfully present" individuals to leave the U.S., importantly, IIRIRA does not make it illegal for any such individual to attend college or for a college to enroll such a student. Rather, it reinforces provisions in the Personal Responsibility and Work Opportunity

[3] Illegality" refers only to the positivist law notion of a residence status which exists outside the legal constructs of a given nation-state. My use of the term does not imply that "illegality" is a personal trait or a value (Dabach 2015; see also de Genova 2002).

Reconciliation Act of 1996 (PRWORA) that do not allow undocumented students to access federal financial aid and do not allow a state to offer undocumented students access to in-state-resident tuition (ISRT) unless the state makes ISRT available to out-of-state-resident U.S. citizens and legal residents who meet the same residency conditions.

In this paper, I examine whether IIRIRA had an effect on four educational outcomes for undocumented young adults: high-school graduation (for 18-21 year-olds), college enrollment (for 18-24 year-olds), associate's-degree attainment and bachelor's-degree attainment (both for 22-30 year-olds). I do so using data from the Current Population Survey Merged Outgoing Rotating Groups (CPS-MORG), and to capture the time period before IIRIRA up until Texas and California enacted the first IIRIRA-compliant ISRT laws in June 2001 and January 2002, respectively. Combined, Texas (14 percent) and California (24 percent) were home to an estimated 38 percent of undocumented residents when each state passed its ISRT law (Passel 2005). Because ISRT laws enable undocumented residents who meet certain conditions (that vary by state) to pay in-state-resident tuition, including observations of undocumented students who were exposed to Texas and California ISRT laws would confound my analysis of IIRIRA's impact on undocumented-student educational attainment. And so, I use data from January 1994 until June 2001.

To proxy for undocumented youth, I focus on foreign-born non-citizen (FBNC) youth of Mexican ancestry, which has been established as the best and most commonly-used approximation for the likely undocumented in available national data sets. In my difference-in-differences research design, I subtract a pair of mean differences to estimate the causal impact of IIRIRA on the educational outcomes of undocumented youth of

Mexican ancestry. For the first difference, I compare outcomes before (January 1994-June 1998) and after (July 1998-June 2001) IIRIRA was enacted. This difference indicates whether there is a possible change in an educational attainment outcome for likely undocumented students that could be possibly associated with IIRIRA. Because this first difference might also reflect the impact of events and conditions which could influence attainment outcomes independently of IIRIRA, I compare the same outcome before and after IIRIRA for a comparison group of U.S. citizens of Mexican ancestry, who are likely to have faced the same non-IIRIRA-related conditions as likely undocumented students, but are not likely to have been affected by IIRIRA provisions regulating undocumented people. I subtract the second difference from the first difference to remove non-IIRIRA-related effects from my estimation. The resulting difference-in-differences is the plausible estimator of the causal effect of IIRIRA on undocumented students' educational attainment.

I find that after IIRIRA, likely undocumented young adults ages 18-21 were two-and-a-half times less likely to graduate from high school, and that likely undocumented young adults 22-30 were four times less likely to hold an associate's degree or higher than their U.S.-citizen peers with whom they had grown up, and four-and-a-half times less likely to hold a bachelor's degree or higher in comparison to their U.S.-citizen peers after IIRIRA, during the years 1998-2001. Seemingly against prediction, IIRIRA is not associated with a decline in college enrollment. It is unclear whether that is due to the relatively low college enrollment rates among the reference group of U.S. citizens of Mexican ancestry. Whatever the reason, the significance and magnitude of diploma and

degree findings are sufficiently distressing for undocumented young people who exhibit no differences from their citizen peers in seeking educational opportunity.

Prior research suggests that the cost of higher education is the greatest barrier to college access in the U.S. (Baum, Ma and Payea 2013; Deming and Dynarski 2010). In the absence of both federal financial aid and ISRT, undocumented students often have to pay tuition rates that equal 88 percent of their family annual median income (compare Baum et al. 2013, with Passel and Taylor 2010). Many undocumented students and their families are simply unable to meet these higher costs (Flores 2010; Gonzales 2011; Oliverez 2006). Those that are able often do so by working multiple jobs to finance their education above and beyond jobs they work to contribute to their family's income (Contreras 2009; W. Perez et al. 2009). Suárez-Orozco et al. (2015) note that meeting these additional financial hurdles is associated with greater neglect of school work and relationships with colleagues, professors, and institutional staff who might serve as a support system, as well as overall higher levels of stress, anxiety, and depression. These costs compound the influence of existing challenges that undocumented students face, including attending poorer resourced high schools, living in neighborhoods with higher concentrations of poverty and cultural isolation, and being more likely to be a first-generation college student, (Gonzales 2009; Potochnick 2014; C. Suárez-Orozco, Yoshikawa, Teranishi and Suárez-Orozco, M. 2011).

To hypothesize how 1.5-generation undocumented youth negotiate their residency status when making college enrollment and participation decisions, I extend Menjívar's (2006) liminal legality framework, which Abrego (2006; 2010 (with Gonzales)) and Gonzales (2008) have developed with others. Building on Gonzales (2016), in particular,

I investigate whether for undocumented students, the greatest barrier to college access might be their residency status. To the best of my knowledge, this study is the first to explore IIRIRA's effect on college-going behaviors. Understanding IIRIRA's effect is important because IIRIRA is the federal law that frames the labor conditions under which undocumented residents live. By limiting access to federal financial aid and ISRT, IIRIRA limits access to a college education for these youth and young people. Consistent with Gonzales (2009; 2011; 2016) and others, I maintain that IIRIRA imposes such substantial inflexible burdens on undocumented residents' ability to secure employment, earn a living wage, and travel freely that for youth transitioning to adulthood, their legal status becomes a "master status" that determines their lives above-and-beyond their skills, education, and linguistic and cultural fluency. This might have had the result of reducing the benefit of a college education for undocumented degree holders (cf. Gonzales 2016).

My findings have implications for understanding how federal laws impact college-aged undocumented youth and laws on education rights. In so doing, I seek to shift the conversation about undocumented students' college access toward a richer discussion of how law creates uniquely burdensome conditions for these youth at key transitional moments in late adolescence that are different from their U.S.-born and foreign-born peers, and how law might ameliorate the same. I elaborate in the conclusion. This paper proceeds as follows. Section II summarizes the background of laws affecting education of the undocumented in the U.S., provides a summary legal-history analysis of IIRIRA, and details how this study aids in understanding tuition and aid policies. Section III describes the theoretical framework motivating this study. Section IV describes the

research design, including data, analytical strategy. Section V discusses results and threats to validity, and Section VI concludes with implications.

II. Background and Literature Review

<INSERT FIGURE 1 HERE>

The current legal and policy situation facing undocumented students' access to higher education has its origins in 1982, when the U.S. Supreme Court ruled in *Plyler v. Doe* (1982) that undocumented students are entitled to the same free, public high-school education as their citizen and authorized-resident peers. Though the Court has not extended *Plyler* to higher education, debates on how to educate undocumented students gained in intensity in the late 1980s and early 1990s, particularly as states struggled with higher costs generally and lower levels of federal support for public education. As shown in Figure 1, during this time, the federal government passed two major immigration reform bills: the Immigration Reform and Control Act of 1986 (IRCA) and the Immigration Act of 1990. IRCA allowed certain undocumented immigrants to gain authorized status and outlined provisions for controlling access to work opportunities for the remaining undocumented. The Immigration Act of 1990 revised visa-issuance policies and clarified penalties on employers who hired undocumented workers. However, neither settled the issue of higher-education access nor provided clarity on whether undocumented students qualified for federal financial aid (U.S. Department of Education, 1989-1990 in Manuel 2014). Debates on whether states should educate undocumented students and to what level reached a zenith in 1996, with states filing numerous federal lawsuits seeking to resolve the issue and Congress passing the Personal

Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) and the Illegal Immigration Reform and Immigrant Responsibility Act of 1996's (IIRIRA), which, while not barring undocumented students' access to higher education, makes attending college effectively cost prohibitive by denying them access to lower in-state tuition costs and making it impossible for them to obtain federal financial aid.

From Plyler to IIRIRA

States have generally complied with both the letter and spirit of *Plyler*, and provide undocumented students with a public education without issue. Many have issued policy documents changing general enrollment requirements so as not to require submission of social security numbers or any other documentation that might disclose unlawful residence status (Brickman 2006). Together with the Family Educational Rights and Privacy Act (FERPA, codified at 20 U.S.C. § 1232), which shields information in K-12 students' files from disclosure—even from federal immigration authorities—without parents' consent, many undocumented students go through elementary and early high-school years unaffected—and in many cases unaware—of their residency status (Gonzales 2009). As a result, undocumented children are generally able to access early and middle-grades education at the same rates as their authorized-resident and citizen peers.

This is not to say that states have not attempted to circumvent *Plyler* through the years through legislation, lawsuits, or other policies and practices. Following IRCA, which granted pathways to authorized residence and citizenship for a large number of previously undocumented residents, a backlash began in states, particularly those with high volumes of immigrants, against providing social services, including education, to

immigrants and their communities (Baker 1997). Congress responded to the IRCA fallout by passing the Immigration Act of 1990, which shifted federal immigration policy away from authoring permanent residence toward authorizing more temporary work visas (Baker). Citizens and lawmakers in high-volume destination states remained unsatisfied with the 1990 Act. Between 1993 and 1994, six states initiated lawsuits against the federal government seeking compensation under IRCA for additional costs of educating undocumented students (*California v. U.S.*, 1997 (Arizona and California); *Chiles v. U.S.*, 1995 (Florida); *Padavan v. U.S.*, 1996 (New York); *New Jersey v. U.S.*, 1996; and *Texas v. U.S.*, 1995).

As its case made its way through the federal courts, California citizens passed Proposition 187, also known as the "Save Our State" initiative in November 1994, with 59 percent voting in favor. In a direct challenge to *Plyler*, Proposition 187 would have required students *and their parents* to prove lawful residency status before enrolling in school (Cooper 1995). A district that could not confirm either residency status would have been required to inform federal immigration authorities immediately (Brickman 2006). Though the federal district court immediately enjoined Proposition 187 from going into effect (*League of United Latin American Citizens v. Wilson*, 1995), the highly racial and nativist statewide campaign created both a chilling effect among the undocumented community, and galvanized political organizing among the broader Latino[4] community (Cervantes, Khokha, and Murray, 1995). Other high-volume

[4] "Latino, " alternately "Latin@" and "Latinx, " is a pan-ethnic identifier of people living in the U.S. who were born in a Latin American country or whose parents or ancestors were, and so self-identify. Hispanic, which is often used interchangeably with Latino, is the U.S. government's preferred term for this population, and is the term by which it accounts for this population in national surveys and census. Though I prefer the more inclusive pan-gendered term "Latin@" or the non-gendered term "Latinx," for ease of comprehension, I will use the term "Latino" for men and mixed-gender group and the term "Latina" for women among this group.

immigration states: Florida, Illinois, New York, and Texas took note of Proposition 187 and proposed various unsuccessful forms legislation based thereupon (Brickman 2006).

Though under challenge, access to elementary and secondary education for undocumented students was relatively secure when compared to post-secondary options. Not only has *Plyler*'s guarantee of access to a basic education not been applied to the higher-education context, prior to 1996, the federal government offered no guidance on how states or their colleges should treat undocumented students. In the absence of regulation, public colleges and universities are neither required to enroll undocumented students nor are they prohibited from doing so. Many states took advantage of pre-existing policies that assess different tuition costs based on legal in-state residency status to deny undocumented students lower in-state-resident tuition (Olivas 1986; 2004). The situation in California, where more than half of the undocumented population resided in the 1980s, was even more precarious, with the state Supreme Court granting ISRT and state financial aid in *Leticia A. v. Board of Regents* (1985), and taking it away six years later in *Regents of the University of California v. Bradford* (1990) upon a citizen lawsuit.

One important exception to federal abstention was IRCA itself. IRCA included provisions that amended the Higher Education Act of 1965 to allow, in addition to U.S. citizens and legal permanent residents, persons who can provide "such [ ] documents as the institution determines constitutes reasonable evidence indicating a 'satisfactory immigration status, '" which is defined circuitously as "an immigration status which does not make the student ineligible for a grant, loan, or work assistance…" (100 Stat. 3388 (1986); see also 20 U.S.C. § 1091(a)(5)(amended 1996)). Assisted by U.S. Department of Education guidance, many colleges and universities interpreted section 1091(a)(5) to

allow certain groups of unauthorized immigrants—but not all—to receive federal financial aid (U.S. Department of Education, 1989- 1990 in Manuel 2014).

Taking their cue from public dissatisfaction with IRCA, the 104th Congress shifted debate about federal immigration policy away from a focus on regulating lawful entry toward regulating the undocumented within the U.S.' borders (Olivas 2004). After a lengthy debate, Congress passed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which President Clinton signed in 1996. As part of general welfare reform, PRWORA contains provisions which deny undocumented immigrants access to most public benefit programs—federal (8 U.S.C. § 1611(a)) and state (§ 1621).[5] As of July 1, 1997, PRWORA explicitly excludes undocumented immigrants, as aliens who are not "qualified aliens," from receiving "postsecondary education benefits" from the federal government (§ 1611(c)(1)(B)). PRWORA also requires states to affirmatively pass legislation allowing undocumented persons to receive state or local benefits, including postsecondary education benefits (§§ 1621(c), (d)).

The Passage and Terms of IIRIRA

House Resolution 3610, a comprehensive immigration reform bill that would further limit undocumented immigrants' access to public services, was introduced on June 11, 1996. The majority of the bill which would become IIRIRA passed the House of Representatives two days later on June 13, 1996 (HR 3610, 1996). On September 26, 1996, while the Joint Conference Committee debated how to reconcile the House version

[5] School breakfast and lunch programs, the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC), certain emergency disaster relief services, and public services "necessary to protect life and safety" are generally made available under PRWORA regardless of immigration or residency status (8 U.S.C. §§ 1611(b)(1)(A), 1615). Existing research sends mixed signals as to undocumented families' participation in such programs (compare, e.g., Leigh and Medel-Herrero 2015 (no difference in WIC participation by immigration status), with Castañeda and Melo 2014, and Yoshikawa 2011 (describing reluctance by mixed-status families to seek available health-care and social services)).

of IIRIRA with the version the Senate passed on July 18, 1996, California Congressman Elton Gallegly introduced a version of California Proposition 187 as a possible amendment (HR 4134, 1996). The Gallegly Amendment would have preempted *Plyler* by allowing states to deny undocumented youth access to public K-12 education or charging them tuition to enroll (Gimpel and Edwards 1999). When the House of Representatives passed the Amendment by a 257-163 vote, it threatened the passage of the overall bill as President Clinton threatened to veto any version of IIRIRA that included the provision (Gimpel and Edwards). In the last hours of the 107th Congress, a bicameral committee removed the Amendment in reconciliation, and the law passed for President Clinton's signature. Evidence from the brief three-month lead-up and debate over IIRIRA suggests that the law passed for political reasons unrelated to—and in fact despite—the undocumented and Latino communities' advocacy, which lends support for IIRIRA's exogeneity with respect to undocumented college-aged youth (Gimpel and Edwards, 1999).

While K-12 restrictions did not survive in the final bill, postsecondary provisions did. Under IIRIRA, as of July 1, 1998:

> Notwithstanding any other provision of law, an alien who is not lawfully present in the United States shall not be eligible on the basis of residence within a State (or a political subdivision) for any postsecondary education benefit unless a citizen or national of the United States is eligible for such a benefit (in no less an amount, duration, and scope) without regard to whether the citizen or national is such a resident.

(8 U.S.C. § 1623 (1996)). IIRIRA amended the Higher Education Act to remove status-determination discretion from colleges (110 Stat. 3009-673 (1996), codified at 20 U.S.C. § 1091(g)(4)(B)(i))). For all students seeking financial aid, colleges are now required to submit proof of said student's lawful immigration status directly to U.S. Citizenship and

Immigration Services (USCIS)[6]. IIRIRA also strengthened federal monitoring and enforcement against employment of the undocumented (110 Stat. 663-665 to 669, codified at 8 U.S.C. §§ 1324a, 1360), and effectively deputized state law enforcement agencies with broad powers to detain and otherwise enforce immigration laws (110 Stat. 663-563, codified at 8 U.S.C. § 1357). While there has been considerable comment on what constitutes a postsecondary education benefit under IIRIRA (compare, e.g., and Hernández 2011 (arguing that ISRT is not a benefit under IIRIRA), with Kim 2012 (arguing that it is)), and whether Congress can regulate the terms under which a state may make a residency determination for allocation of state benefits (see, e.g., Olivas 2004), no individual or state has sued for allowing undocumented students access to in-state-resident tuition. Congress's actions in passing IIRIRA are generally believed to be squarely within its plenary power over immigration (cf. *Plyler 19*82), over which the federal courts are generally reluctant to interfere (but see *Zadvydas v. Davis* 2001 (finding that the plenary power does not extend to indefinite detention of a stateless immigrant under order of deportation)). Further, the evidence clearly establishes that no state allowed undocumented students ISRT from between IIRIRA's effective date until June 16, 2001 when Texas passed its ISRT bill (TEX. EDUC. CODE ANN. § 54.052 (Vernon 2001)).

IIRIRA's Potential Effects on Undocumented Students' Educational Attainment

IIRIRA possibly impacts undocumented youth and young adults in at least three important ways. First, as stated above, IIRIRA made attending college—one of the few

[6] Immigration and Naturalization Services (INS), an agency of the Department of Justice, was the predecessor agency to U.S. Citizenship and Immigration Services (USCIS). INS transferred immigration responsibilities to USCIS on March 1 2003 pursuant to the Homeland Security Act of 2002 (6 U.S.C. § 1, et seq.).

activities undocumented residents over the age of 18 are allowed in the U.S.—more costly both by raising their actual tuition costs and creating structures which reinforced PRWORA's provisions which barred undocumented students from receiving federal financial aid.[7] Second, IIRIRA lowers the Government's burden to prove that an immigrant is unauthorized and deportable. This is believed to have increased school withdrawals and truancy among undocumented high-school students and reduced parental engagement with schools out of fear of detection and deportation (Olivos and Mendoza 2010; but see Gonzales, Heredia and Negrón-Gonzales 2015). Third, IIRIRA made it more difficult for employers to hire undocumented workers by requiring employers to verify the residency status of all workers using new, streamlined processes, and increasing employer penalties for failures to comply (Moore, Shulock and Jensen 2009 in Gonzales 2009).

To the best of my knowledge, there does not appear to be any empirical literature exploring IIRIRA's potential effects on undocumented students' educational attainment. Leading scholarship on IIRIRA is largely jurisprudential in nature, focusing primarily on issues of the constitutionality and practicality of those laws (e.g., Hernández 2011; Manuel 2014; Kim 2012; Olivas 2004; 2009; Sinodis (2011)). Missing from this discussion is any evidence that PRWORA and IIRIRA *actually* caused any hypothesized decline in undocumented students' college-going behaviors or if it had any spillover effects on high-school graduation.

While estimates of the number of undocumented students who have taken advantage of ISRT policies are generally unavailable, existing studies generally show that

[7] States were already prohibited from providing federal financial aid to undocumented immigrants pursuant to the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 signed one month earlier ("PRWORA, " 110 Stat. 2105; see also 8 U.S.C. § 1611).

ISRT policies like Texas's, which some states have enacted purportedly to counteract the effect of IIRIRA, decrease undocumented-student high-school dropout and increase undocumented-student college enrollment.[8] Examining CPS-MORG data and using FBNC as a proxy for the undocumented, Potochnick (2014) finds that ISRT reduces high-school dropout rates among Mexican FBNC youth ages 16- 19 by 20 percent. Bozick and Miller (2014) estimate comparative high-school enrollment outcomes among Mexican FBNC youth living in states that specifically deny ISRT to undocumented college students to similar youth living in states that allow ISRT, with states with no specific policy as a control. They find that youth living in restrictive states are 49% less likely than youth in control states to be enrolled in high school, and youth in facilitative states are 65% more likely to be enrolled in high school.

Kaushal (2008) finds that ISRT improves college enrollment, associate-degree attainment, and the proportion of undocumented individuals with some college education. Flores (2010) finds that undocumented students are 1.54 times as likely to enroll in college following ISRT; Amuedo-Dorantes and Sparber (2012) reach similar findings.[9] Cortes's (2013) findings that the gaining legal residency status led to a 25 percent increase in formerly undocumented student's college enrollment likelihoods. Though looking at two different sets of laws in two different time periods, these studies on IRCA and ISRT suggest that undocumented students are more likely to enroll in—and perhaps graduate from—college in the absence of legal and financial barriers (Cortes 2013).

[8] See Appendix 1 for a list of ISRT policies by state.
[9] Chin and Juhn (2011) examined American Community Survey data and found no effect. The authors acknowledge that their no-effect finding might be due to the relative newness of the ACS dataset. It was first collected in 2000—ahead of the analytic time period of interest in this study—and the relatively short pre- and post-policy periods they could measure in their analytic window 2001-2005.

Conversely, these students are less likely to enroll and graduate from college when these barriers are present or enhanced.

III. Theoretical Framework and Hypotheses

My approach is informed by two major theoretical frameworks, liminal legality and Becker's human capital theory as applied to college enrollment. The liminal legality framework has the advantage of explaining the environment in which undocumented young adults make decisions about their education. Meanwhile, Becker's Human Capital theory contributes a framework for understanding how they make those decisions.

Liminal Legality

I hypothesize that legal barriers are uniquely experienced by undocumented youth, most particularly by the 1.5 generation, and most intensely as they transition into adulthood (Gonzales 2011; and Chavez 2012). Having migrated to the U.S. as young children, the U.S. is the only country many remember (Gonzales 2011). Undocumented youth grow up here, attend school, and develop culture and language alongside their citizen peers (Abrego 2006; Menjívar 2006). These youth enjoy social and cultural citizenship, which before adulthood is practically indistinguishable from the participation rights of their childhood friends (Torres and Wicks-Asbun 2014). As children, many are unaware of their residency status, if not shielded by laws from its direct consequences (Gonzales 2009; 2016).

As they approach adulthood, undocumented youth grow beyond FERPA's schoolhouse protections and for the first time face barriers to work and freedom as they are unable to secure verified employment, driver's licenses, or bank accounts without

authorized documentation (Gonzales and Chavez 2012). Many students discover their residency status for the first time as they apply for and get into college, but are denied access to federal financial aid programs, and in non-ISRT states to in-state-resident tuition (Gonzales 2016). Unbeknownst to many, they also face the expiration of IIRIRA tolling provisions that suspend a legal determination of "unlawful presence" until six months after a resident's 18th birthday (8 U.S.C. § 1182(a)).

Paradoxically, this final passage into legal adulthood occurs simultaneously with a jarring passage into "illegality."[10] The "legal limbo" (Menjívar 2006) these young people emerge into is rife with uncertainty that foreclose many opportunities they once believed were available to them, shift their perceptions about those that remain, and alter their plans for their future. For youth who, often unbeknownst to them, grew up shielded from the consequences of their status, this abrupt shift into "illegality" is metastatic. The specter of deportation of oneself and one's family members often leads to a withdrawal from communities of childhood and reluctant introduction into unfamiliar communities gathered around surviving a clandestine existence (Gonzales 2016). Poorer health care options, low prospects of earning a livable wage, increased vulnerability to the whims of local bureaucrats, and reduced physical and psychological safety converge to make "illegality" a master status that determines life outcomes above and beyond otherwise salient factors (Gleeson and Gonzales 2012; Terriquez 2015; Gonzales 2016). In few arenas is this clearer than in high-school-diploma attainment and college decision moments.

Theories about College Decisions

[10] "Illegality" refers only to the positivist law notion of a residence status which exists outside the legal constructs of a given nation-state. My use of the term does not imply that "illegality" is a personal trait or a value (Dabach 2015; see also de Genova 2002).

According to Becker's (1993) college decision model, an individual student chooses whether to invest in college through a thought process in which he compares his expectation of the benefits of attending college to his expectation of the costs. When the expected benefits exceed the expected costs, a person will choose to enroll as a maximization of his utility (Becker 1993).

In addition to differences in how individuals respond to uncertainty in the costs or benefits from college (Altonji 1993 in Flores 2010), access to adequate or accurate information (e.g., Bettinger, Long, Oreopoulous and Sanbonmatsu 2012), ability to process information (Conlisk 1996), emotional or psychological stress (C. Suárez Orozco et al. 2015), income and attendant credit constraints (see Avery and Hoxby 2004), previous educational experiences (Gonzales, Heredia and Negrón-Gonzales 2015), time and other resources (Chetty 2015), and perhaps most poignantly the threat of discovery and deportation (Gonzales and Chavez 2012) constrain both the choices available to an undocumented young adult and how he approaches this decision (Simon 1979). While liminality is generally understood to restrictively moderate college enrollment constraints, there is also evidence that it encourages college persistence among a subset of undocumented youth who see going to college as one of the few activities they are allowed under current law (Gonzales 2016) and higher educational attainment as providing opportunities for formal and cultural legitimation above and beyond possible improved financial prospects (Contreras 2009; Diaz-Strong and Meiners 2007; C. Suárez-Orozco et al. 2015; Torres and Wicks-Asbun 2014).

Hypotheses

Because IIRIRA raised the cost of a college education for undocumented students and reduced the benefits of having such an education on their job prospects, I hypothesize that fewer undocumented students will enroll and graduate. While the difference between out-of-state and in-state-resident tuition is greater for four-year colleges than the two-year colleges most undocumented students are believed to attend (see Gonzales 2007), students who attend two-year colleges are known to be more sensitive to price than their four-year counterparts (Kane 1999); undocumented students even more so (Dougherty et al. 2010). I expect IIRIRA to have a stronger impact on college outcomes as they progress because the compounding nature of "illegality" on increasing the costs of obtaining an education and decreasing the benefits of attaining a college degree. Over time and as adolescence crystalizes into adulthood, IIRIRA might enhance the effect of a liminal existence in terms of college, psychological, family, and community costs. With time and at later life stages, liminality might make permanent actual and perceived uncertainties about post-college employment, which, against compounded costs, might weigh in favor of stopping out or ending college pursuits altogether

I hypothesize that IIRIRA has a more limited effect on high-school graduation because a high-school diploma is free. However, IIRIRA could still have an impact on high school graduation because restrictions on college access and employment disincentivizes educational attainment for undocumented youth. Undocumented youth are also shielded from many of the consequences of their residency status until they approach graduation due to *Plyler*, FERPA, and community protective factors, which may reduce IIRIRA's effect on this outcome.

With this integrated framework in mind, I investigate whether IIRIRA has an impact on undocumented students' educational attainment by asking what effects do simultaneously removing federal-financial-aid and in-state-resident-tuition eligibility have on:

(1) high-school-diploma attainment for likely undocumented students of Mexican descent ages 18-21;

(2) college enrollment for similar students ages 18-24;

(3) associate's-degree attainment; and

(4) bachelor's-degree attainment both for similar students ages 22-30, compared to their age-cohort U.S.-citizen peers of Mexican descent.

IV. Research Design

A. Data

Following research on ISRT effects on high-school and college behavior (see, e.g., Amuedo-Dorantes and Sparber 2014; Flores 2010; Flores and Chapa 2008; Kaushal 2008; Potochnick 2014), I use individual-level data from the CPS-MORG for the years 1994-2001 in my analyses, excluding observations taken after the State of Texas enacted the first ISRT law in June 2001. These data contain information about educational attainment, enrollment, national origin, sex, marital, and citizenship/residency status from a multistage stratified sample of approximately 60, 000 households per month (U.S. Census Bureau 2015). I supplement CPS-MORG data with U.S. Bureau of Labor Statistics (BLS) data on monthly labor market conditions over the same seven-year time period.

The CPS samples by state-level clusters and by primary-sampling-unit strata within states; and weights by a complex formula that takes age, sex, race, and ethnicity into account to approximate national population representativeness (Judson and Swanson 2011; Schmidley and Robinson 2003). Because of these features, many scholars who examine CPS data use the provided weights to correct sampling bias. However, CPS weights do not account for national origin or citizenship, which are the most important variables for estimating demographic change or related effects on the undocumented population (Van Hook, Zhang, Bean and Passel 2006). Further, no governmental agency separately counts the undocumented. As a result, the CPS undercounts the foreign-born and the undocumented even more so. Despite differences between my population of interest and the U.S. population for which weights were derived (see Davern, Jones, Lepowski, Davidson and Blewett 2007), weights might prove useful in generalizing results using these data to the U.S. population.

Though the CPS does not count the undocumented separately, it is still useful because it inadvertently includes undocumented immigrants among its sample of foreign-born non-citizens (FBNC). Because CPS does not ask about residency status, there is no direct inducement for respondents to lie about their citizenship or country of origin. One important consequence of this might be more undocumented people answering the survey than there would be if there was a clearer threat of disclosure (cf. Passel and Cohn 2009). Further, CPS data also contain information about individual and parental nativity, and citizenship status that can be used to proxy for undocumented status. As a result, this survey is one of the most important governmental data sources for information about my population of interest because of information it contains about individual and parental

nativity, and citizenship status (Passel 2005). The only other data source known to capture observations of undocumented residents is the American Community Survey (ACS), which was first taken in 2001 after IIRIRA was enacted.

B. Measures

1. Outcome Measures

My binary (1 = yes, 0 = no) outcome measures are: (1) having a high-school diploma ($HSDIPLOMA$), (2) being enrolled in college ($COLLENROLLED$), (3) having an associate's degree or higher ($ASSOCIATES$), and having a bachelor's degree or higher ($BACHELOR$).

2. Policy Effect Measure

My policy effect measure is a difference-in-difference estimator, the interaction of two binary measures, $FBNC \times IIRIRA$. The first of these, $FBNC$, measures all foreign-born non-citizens observed in a sample. This is the treatment assignment proxy for being undocumented (1 = yes, 0 = no). After Passel (2005; and Cohn 2009; and Taylor 2010), I adapt the "residual method" to available data in estimating $FBNC$. This is a conventional method for estimating the foreign-born in the absence of data of residents who do not have legal documents (Deardorff and Blumerman 2001; Judson and Swanson 2011; Schmidley and Robinson 2003). Consistent with this method, I subtract from the total foreign-born population persons ($ForeignBorn$) all who indicate: (1) citizen status ($Citizen$) by (a) birth in a U.S. territory[11], (b) one or both parents' citizenship or birth in the U.S. or a U.S. territory[12], or (c) citizenship in the U.S., presumably by naturalization;

[11] Guam, the Panama Canal Zone (until 1999), Puerto Rico, and the U.S. Virgin Islands (see 8 U.S.C. § 1401).

[12] CPS-MORG data does not provide information about whether an individual's parents were married or if s/he was born in wedlock. Without this information, there is potential measurement error in the estimation

or (2) citizenship or birth in Cuba[13] ($Cuban$). The remainder, or residual per Passel et al.'s parlance, are all foreign-born non-citizens (FBNC), the category that contains undocumented respondents.

(1)

$$FBNC = ForeignBorn - Citizen - Cuban$$

The strongest proxy for undocumented status in the CPS is being FBNC and of Mexican origin (Kaushal 2008; Potochnick 2014). Passel and Cohn (2009) estimate that around 56 percent of FBNC Mexicans are undocumented. Among FBNC Mexicans who have lived in the U.S. for fewer than 10 years, 80 percent are estimated to be undocumented (Passel and Cohn 2009, in Potochnick 2014). Undocumented Mexicans account for approximately 59 percent of the U.S. undocumented, with non-Mexican undocumented accounting for another 22 percent (Passel and Cohn 2009). These shares have remained constant for the last three decades (Passel and Cohn).

Despite these efforts, the $FBNC$ variable inadvertently includes foreign-born non-citizens lawfully resident in the U.S. on various green cards, visas, refugee, or other temporary status. As a consequence, estimates of undocumented-student effects using these data are downwardly biased. That is, it is more difficult to find a policy effect on undocumented persons than it would be if one could distill the FBNC categories to

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of who is a lawful foreign-born resident (see *Nguyen v. INS* (2001), the U.S. Supreme Court upholding portions of the Immigration and Nationality Act of 1952, codified at 8 U.S.C. § 1409, that maintain more restrictive citizenship and residence requirements for children born out of wedlock to a foreign mother and a U.S. father than for out-of-wedlock children born to a foreign father and a U.S. mother). However, the error that would be introduced by not identifying a foreign-born child of a U.S. citizen parent as having lawful right to residence is far greater than that which might have been introduced

[13] The Cuban Adjustment Act of 1966, codified at 8 U.S.C. § 1255, generally allows Cubans to legally emigrate to the U.S. and establish residency. Though the Cuban Migration Agreement of 1996 allows the U.S. to repatriate any Cubans who fail to reach U.S. territory in their attempt to emigrate, successful migrants are typically allowed to adjust and, after one year, are eligible for permanent resident status under the 1966 Act.

remove non-citizen lawful residents. This is a common, well-noted limitation that exists, frankly, out of the very status and stigma of being undocumented (see Amuedo-Dorantes and Sparber 2014; Chin and Juhn 2011; Flores 2010; Kaushal 2008; Potochnick 2014 (all evaluating post-IIRIRA state-level in-state-resident-tuition laws)).

Kaushal (2008) provides a formula to estimate a rough effect size of the bias:

(2) $$Bias = Lawful + [(FBNC - Lawful) * UndercountRate]$$

Estimating that approximately 20 percent of Mexican FBNC are in fact lawful residents, and acknowledging Passel's (2005) estimates that the FBNC measure undercounts Mexican undocumented by around 10 percent, any policy effect calculated from a measure using Mexican FBNC as a proxy for the undocumented absorbs an 28 percent downward bias (Kaushal 2008). To adjust for this bias, a truer predicted policy effect would be 1.28 times an estimated value (Kaushal 2008; Potochnick 2014). Despite this coefficient correction, this unavoidable measurement error increases the standard error, and with it the likelihood of Type II error (Wooldridge 2012). As a result, I am more likely to fail to reject the null hypothesis when there is an actual policy effect.

The second measure in the difference-in-differences interaction, $IIRIRA$, indicates whether a person was surveyed after July 1, 1998, when IIRIRA went into effect (1 = yes, 0 = no).

C. Sampling

To account for identifying the undocumented only among the eligible foreign-born population of Mexican origin, I restrict my samples to respondents of Mexican origin. These are survey respondents who indicate: (1) birth in Mexico; (2) both parents' birth in Mexico; (3) one parent's birth in Mexico and the other's birth outside of the U.S.,

its territories, and Cuba; or (4) Mexican ancestry. While this methodological adjustment allows me to reduce the effects of any measurement bias associated with ethnicity and country of origin that might otherwise affect my observations, it also eliminates the influence of ethnic and national-origin variation on my analyses thereby permitting a more apples-to-apples comparison of youth who are more likely to have shared similar peers and educational experiences, be of similar socioeconomic backgrounds, and have experienced similar responses to structural and cultural aggressions and discriminations, including a presumption of illegality (see de Genova 2004).

Given this, for my analyses of high-school diploma attainment, I include all CPS-MORG respondents of Mexican ancestry between the ages of 18 and 21; and similar respondents between the ages of 18 and 24 for college enrollment and degree attainment outcomes.

D. Analytic Strategy

I use a difference-in-differences strategy to estimate IIRIRA's causal effect of undocumented-student educational attainment outcomes. As a "first difference, " I calculate the difference between the outcome for undocumented youth before IIRIRA's enactment and the same outcome after IIRIRA's enactment. This difference estimates the likelihood of a given outcome for the undocumented associated with IIRIRA. I subtract from this a "second difference, " the average difference in outcome for a control group of U.S. citizen co-ethnic youth. This difference accounts for non-IIRIRA related changes in outcome, including common factors which might explain differences in outcome for undocumented youth apart from IIRIRA. The remaining difference is an intent-to-treat estimate (Murnane and Willett 2010; Wooldridge 2012).

This strategy is equivalent to fitting the following equation:

(3)

$$y_{ist} = \beta_0 + \beta_1(FBNC \times IIRIRA_{ist}) + \beta_2(FBNC_{ist}) + \beta_3(IIRIRA_t) + \beta_4(X) + \beta_5(Z) + state_s + year_t + month_t + stateyear_{st} + e_{ist}$$

In Equation (3), y_{ist} represents the log-odds of a given binary outcome for an individual i living in state s at time t. $FBNC \times IIRIRA_{ist}$ is my main predictor of interest: an individual i being a foreign-born non-citizen living in state s on or after time t when ISRT was enacted. β_1, thus, is the causal effect estimator of IIRIRA's effect on an educational attainment outcome. β_2 estimates the average before-IIRIRA difference in log odds for $FBNC$, a foreign-born non-citizen from a citizen on a given outcome. β_3 estimates the average difference in log odds for a citizen surveyed after $IIRIRA$ from a citizen surveyed before IIRIRA on a given outcome. β_4 measures adjusted differences in log-odds along the covariate matrix X, which measures time-varying individual characteristics such as age, gender, marital status. β_5 measures adjusted differences in log-odds along the covariate matrix Z, which measures time-varying state characteristics such as general and community education attainment trends, migration trends, and unemployment rate. I include time-invariant state fixed effects to account for between-state variation, state-invariant year effects to account for between-year variation, and month fixed effects to account for variation in educational attainment across an academic year; as well as state-by-year year fixed effects to account for state trends which may impact educational attainment. e is an error term.

E. Sensitivity and Falsification Checks on Threats to Validity

This is a quasi-experimental, and as such is limited in its ability to support causal findings (Shadish, Cook and Campbell 2002). It is also vulnerable to particular threats of validity that I address through a number of sensitivity and falsification analyses. The greatest threat to validity comes from introduced, but unavoidable error measuring who is undocumented. In the absence of direct measures, I am forced to proxy undocumented status based on what is known about period of entry, national origin, age, parental origin, and their associations with the likelihood of being undocumented (Judson and Swanson 2011). While residual estimation yields a measure that inadvertently contains documented non-citizens as well as undocumented ones, my proposed measure is the strongest proxy we can identify for undocumented status from governmental data. To test the strength of this proxy, I test differences in educational attainment outcomes among U.S. citizens by foreign birth. If I have accurately identified the subgroup which contains undocumented youth, I should not observe policy effects of similar magnitude, if any at all, targeted toward this population within the foreign-born citizen group.

I also test whether my estimate is truly capturing an IIRIRA policy effect on the likely undocumented by testing for an effect on non-Latino foreign-born non-citizens and non-Latino whites of similar ages. If I have truly identified a policy shock unique to Mexican FBNC, I should not observe either no effect or a substantially smaller effect among any of these groups. I include an average state-month measure of Mexican FBNC residents to account for demographic fluctuation, and measures of educational attainment for white adults of the previous generation to account for general state education trends and for Mexican adults of the previous generation to account for community education

trends. Finally, as discussed above, I include state-by-year fixed effects in a model to account for otherwise unaccounted-for state-specific trends, such as the presence of anti-affirmative-action policies (see Flores 2010) or differences in local immigration enforcement (Schuck 2000) which might contribute to educational attainment differences above and beyond what one might expect given IIRIRA.

F. Limitations

In addition to the limitations posed by quasi-experimental analytic methods I discussed above, certain data limitations limit my ability to more fully understand and explain how IIRIRA impacts educational attainment for likely undocumented youth. First, until 1998, the CPS-MORG did not distinctly measure GED attainment from attainment of a traditional high-school diploma. I am unable, thus, to measure any differences in IIRIRA's impact on GED. Murnane (2013, 17) notes that trend estimates of high-school diploma attainment of Latino youth are "particularly sensitive" to whether a GED credential is treated the same as a traditional high-school diploma or differently. Because of the large increase in GED recipients during the 1990s and the greater likelihood of Latinos with a secondary-school credential to have a GED as compared to non-Latino Whites, this data limitation introduces a possible confounder in my analyses of IIRIRA's effect on high-school-diploma attainment.

Second, CPS-MORG data do not distinguish between enrollment in a two-year college and a four-year college. We have reason to believe that undocumented college students are more likely to be enrolled in two-year rather than four-year colleges (Gonzales 2007). It is possible that IIRIRA policies influence undocumented student college enrollment behaviors differently by type of college. Because of this limitation,

however, I am unable to fully evaluate how IIRIRA might have contributed to this disproportionality. Third, the CPS-MORG only asks enrollment questions of 16-24 year olds. This is a significant limitation because it fails to capture enrollment behaviors of non-traditional college students, among whom are a disproportionate number of undocumented immigrants (Diaz-Strong, Gomez, et al. 2011). Fourth, related to this, these data do not capture important information such as college type, number of classes enrolled in, tuition assessed, or distance between home and college that might weigh against IIRIRA effects on enrollment and degree attainment. Fifth, though the CPS captures more detailed individual- and family-level education and interstate migration measures in its annual October Supplement, these measures are unavailable and un-imputable to its MORG files which contain the best proxy measures for residency status.

Sixth, a final notable limitation is that these are cross-sectional and not longitudinal data. If these were longitudinal data, I might be able to measure individual-level stop-outs and start-backs, and capture the effects both of tuition policy on reducing or increasing the time gaps between college attendance and of financial aid policy in reducing or increasing the need to stop-out to raise college costs. Despite these limitations, this paper provides important evidence as policy analysts, educators, and lawyers try to understand the impact of access-facilitating laws on undocumented-student college enrollment and degree attainment.

V. Results and Discussion

A. Univariate Analyses

<INSERT TABLE 1 HERE>

To begin my analyses on IIRIRA's effects on educational outcomes, I perform adjusted Wald tests comparing, by citizenship status, the mean differences before and after IIRIRA's effective date in 18-21 year-olds having a high-school diploma, 18-24 year-olds being enrolled in college, and 22-30 year-olds having an associate's degree or higher and having a bachelor's degree or higher. In Table 1, I report the means and robust standard errors for these outcomes as well as individual-level covariates: age (in years), percentage male, and percentage married separately by age-cohort panels. I also report, as state-level characteristics, means and robust standard errors for differences in: monthly unemployment rate, as a measure of monthly changes in labor market conditions; monthly percentages of Mexicans and Mexican foreign-born non-citizens surveyed, as demographic control measures; and monthly percentages of non-Latino whites ages 30-45 with some college education and with a bachelor's degree or higher as a proxy for education trends (see, e.g., Flores and Chapa 2008; Potochnick 2014 (for similar descriptive analyses)).

Within the FBNC treatment panels, there are few superficially statistically significant differences in educational attainment before and after IIRIRA. Though there is a significant decline in the number of 18-24 year-old respondents enrolled in college (5.22 vs. 4.45 percent, $p = 0.034$), the declines among 22-30 year-old respondents reporting having an associate's degree or higher (3.96 to 3.81 percent, $p = 0.729$), and having a bachelor's degree or higher (2.42 to 2.33 percent, $p = 0.753$) are not significant,

and the rates of high-school diploma attainment among 18-21 year-old respondents, though also not significantly different, are better after IIRIRA (31.20 vs. 32.95, $p = 0.435$). At the same time, there are statistically significant improvements among citizen youth in associate's (11.14 to 15.77 percent, $p < 0.001$) and bachelor's degree attainment (6.31 vs. 7.79, $p = 0.002$). Citizen youth also experience a decline in college enrollment after IIRIRA, though unlike with FBNC youth, this difference is not statistically significant (9.94 vs. 7.10, $p= 0.205$). High-school diploma attainment among citizen youth improves after IIRIRA, but not significantly so (57.13 vs. 60.31, $p = 0.250$). Taken together, these preliminary results might suggest that IIRIRA stalled otherwise expected growth in likely undocumented students' college degree attainment, and possibly in college participation. Importantly, these results suggest that factors other than changes in the number of high-school graduates in the college pool are leading possible declines in college participation among FBNC youth of Mexican ancestry.

One possible factor is differences in labor market conditions. Higher unemployment rates have long been associated with increased community college enrollments (Betts and MacFarland 1995; Hillman and Orians 2013; Pennington, McGinty and Williams 2002), which is where most undocumented college-goers enroll (Gonzales 2007; P. Perez 2010). Inversely, positive labor market conditions, such as lower unemployment, are associated with lower college enrollments. Latino youth, especially undocumented Latino youth, are known to be particularly sensitive to local unemployment conditions (see Fry and Taylor 2013).

The labor market improved in the mid-1990s, and as a result I observe lower unemployment factors facing both FBNC and citizen respondents in all three panels.

Unemployment rates dropped for FBNC participants in all three panels by 28 percent (18-21: 6.40 vs. 4.62, -28 percent, $p < 0.001$; 18-24: 6.32 vs. 4.52, 28 percent, $p < 0.001$; 22-30: 6.34 vs. 4.58, $p < 0.001$), and for citizen participants between 23 and 25 percent (18-21: 6.05 vs. 4.67, 23 percent, $p < 0.001$; 18-24: 6.07 vs. 4.66, 23 percent $p < 0.001$; 22-30: 6.18 vs. 4.62 $p <0.001$).

In each panel citizen youth are slightly younger, on average, than their FBNC peers. This difference is only statistically significant among respondents ages 18-24 ($p < 0.001$). Because older age is associated with a greater likelihood of having delayed first enrollment in college (Jacobs and King 2002), enrolling and later stopping out (Bradburn 2002), attending college part-time (Bozick and DeLuca 2005), and being less likely to persist to degree attainment once enrolled (Bozick and Deluca), my finding makes age an important factor to account for in assessing IIRIRA's effects on college enrollment.

There are also significantly more men among FBNC respondents than citizen respondents in the 18-21 ($p = 0.033$) and 18-24 year-old panels ($p = 0.007$), and marginally so among 22-30 year-old respondents ($p = 0.074$). Among 18-24 year olds, there are more men after IIRIRA for both FBNC and citizen panels, though only the 18 percent increase among citizens is significant (45.90 vs. 54.29, $p = 0.001$).

This is noteworthy because like age, gender is an important predictor in high-school graduation and post-secondary outcomes, though research suggests it operates in educational contexts differently for Latino youth. Goldin, Katz, and Kuziemko (2006) observe a narrowing gender gap in bachelor's degree attainment among white non-Latino youth from 1979 to 2000. Whereas in 1979, the ratio of female to male bachelor's degree holders was 0.87, by 2000 it had reversed such that the ratio of female to male bachelor's

degree holders was 1.2 (Goldin, Katz, and Kuziemko). Heckman and LaFontaine (2010) attribute much of this shift to a substantial decline in high-school graduation among white men. Even these observations, Heckman and LaFontaine find, only account for half of the current gender gap favoring women in college enrollments and graduation.

Studies on Latino youth suggest an intersectional result. Latinas are more likely than Latinos, but less likely than white non-Latina women to have a high-school diploma (Murnane 2013). At the time IIRIRA was enacted, Latinas were as likely as their male counterparts to enroll in college (Fry and Lopez 2014, but see Vasquez 2002 (finding that Latinas were less likely to enroll in four-year programs)), but were substantially less likely to obtain either a bachelor's degree (Sy and Romero 2008). Espinoza (2010) and Sy and Romero (2008) separately document greater familial and societal expectations among Latinas, and Rosas and Hamrick (2002) among Mexican women, in both cases particularly from first-generation immigrant families, to assist with family caretaking, contribute to the household, and to prioritize family needs over academic pursuits. Similar to the 1957 cohort of white women Goldin, Katz, and Kuziemko (2006) observed, first-generation Latinas in the Espinoza and Sy and Romero studies face compounded pressures that frustrate their higher-education pursuits. Notably, these gaps appear to have reversed profoundly by 2012, with Latinas currently being more likely to attend college than white women (Fry and Lopez 2014). Recent research suggests, however, that these pressures remain steadfast for undocumented Latinas, particularly members of the 1.5 generation whose families rely upon their familiarity with U.S. bureaucracies and social systems to survive (C. Suárez-Orozco et al. 2015).

Related to gender is another possible factor in differences: marital status. In their seminal study, Teachman and Polonko (1988) found that marriage delays college enrollment for both men and women ages 18-25. While they find that men are more likely to enroll in college after two years of marriage, women, Teachman and Polonko also find that marriage is particularly harmful to women's lifetime likelihoods of enrollment and degree completion. While more recent research has challenged Teachman and Polonko with respect to married women over the age of 25 (e.g., Deutsch and Schmertz 2011; Hostetler, Sweet, and Moen 2007; Jacobs and King 2002), the extent to which being married explains college enrollment and degree-completion delays (e.g., Bozick and DeLuca 2005), and even to whether marriage affects Latino college students at all (e.g., Otero, Rivas, and Rivera 2007), the general scholarly consensus weighs in favor of an association between being married and lower likelihoods of enrollment and degree attainment (Bradburn 2002; Harpe and Kaniuka 2012; Leppel 2002; Negy and Meehan 2003).

My observed 20 percent decline among 18-24 year olds in FBNC respondents' likelihood to be married across IIRIRA (38.14 vs. 30.51, $p = 0.014$), is thus noteworthy with respect to enrollment, particularly as I observe no contemporary significant decline among citizens (22.65 vs. 21.50, $p = 0.623$). I observe a larger, 37 percent decline in marital likelihoods among 18-21 FBNC respondents (26.70 vs. 16.83, $p = 0.014$), but I also observe a 20 percent decline among citizen respondents of the same age (15.89 vs. 12.68, $p = 0.013$). While there is a 12 to 15 percentage-point difference in favor of FBNC respondents ages 22-30 being married when compared to their citizen counterparts, there

is no significant difference in these likelihoods across IIRIRA for either group (FBNC: 60.41 vs. 60.64, p = 0.945; Citizen: 45.79 vs. 48.85, p = 0.372).

Two other factors trend toward significance across IIRIRA for FBNC respondents in all three panels: the percentage of Mexican FBNC surveyed in a state during a given month and the percentage of White respondents with a bachelor's degree or higher. After IIRIRA, Mexican FBNC respondents were somewhat less likely to live in a state with other Mexican FBNC (18-21: 7.99 vs. 6.37, p = 0.060; 18-24: 7.79 vs. 6.45, p = 0.057; 22-30: 7.61 vs. 6.83, p = 0.054). There were neither no significant differences in similar likelihoods for citizens across IIRIRA (18-21: p = 0.286; 18-24: p = 0.697; 22-30: p = 0.576) nor were there significant differences between FBNC and citizen respondents in any given panel (18-21: p = 0.886; 18:24: p = 0.819; 22-30: p = 0.464).

A robust literature documents the importance of networks among the undocumented community for providing protective factors, in the form of social, psychological, informational and financial support, that encourage undocumented youth to persist to high-school and college graduation (see, e.g., Abrego 2006; Abrego and Gonzales 2010; Enriquez 2011; Gonzales 2010; 2016; Muñoz and Maldonado 2010; W. Perez et al. 2009). Larger undocumented communities that are more capable of offering this type of network-based support tend to exist in traditional immigrant destinations and are densely concentrated in larger cities (DeSipio 2001). Conversely, smaller undocumented communities tend to be geographically dispersed and in non-traditional immigrant destinations without existing sociopolitical structures that facilitate these protective supports (DeSipio; but see Benjamin-Alvarado, DeSipio, and Montoya 2009

(finding that state-level anti-immigrant legislation catalyzes network development among smaller, geographically dispersed undocumented communities).

Across-state differences in educational trends might also be an important factor. Research consistently observes differences in educational opportunities available to Latino youth, generally, and undocumented youth among them when compared to their white, citizen peers. Latino youth, and the undocumented among them, are more likely to attend poorer resourced schools in terms of facilities, courses available, teacher quality, college counseling, and extra-curricular activities (cf. Gonzales 2010; Perna and Titus 2005; Tornatzky, Cutler, and Lee 2002). Even within these schools, tracking and other resource hoarding might contribute to lower educational attainment rates among undocumented youth (Gonzales 2010). My measures for within-state differences among Mexican-ancestry youth attempt to account for within-state variation at each educational stage. There are no statistically significant differences across IIRIRA for FBNC youth.

However, opportunity and achievement gaps tend to remain constant within states, making state education trends an important control in a multistate analysis. I do so by accounting for the number of white respondents in a state, ages 30-45, who have reached the educational milestone I analyze for Mexican FBNC. In all panels across IIRIRA, there are fewer white respondents who report having a high-school diploma, some college education, and a bachelor's degree.

These results show preliminary support for my hypotheses that IIRIRA adversely affected undocumented students' post-secondary outcomes. The absence of a statistically significant difference in high-school diploma attainment, particularly in the face of significant differences in college enrollment and degree outcomes, lends support to

Abrego (2006) and Gonzales (2009)'s theory that *Plyler* (1982) and laws like FERPA operate to shield undocumented K-12 students from "illegality." The existence, however, of a number of individual- and state-level characteristic differences, both within and across groups, suggest possible confounding—or mediating—variables which must be accounted for in a causal-effect model.

B. Multivariate Analyses

1. High-School Diploma Attainment

<INSERT TABLE 2 HERE>

As I discussed earlier, and I show in Table 2, I estimate the causal impact of IIRIRA on educational attainment outcomes using sets of difference-in-difference logistic regression models. Well against prediction, the enactment of IIRIRA had a profound, immediate effect on Mexican FBNC youth's obtaining a high-school diploma credential. As I display in Table 2, my baseline[14] estimate shows a Mexican FBNC youth between the ages of 18 and 21 being three times less likely than his U.S.-citizen co-ethnic peer to obtain a high-school diploma before IIRIRA (OR: 0.34, $p < 0.000$). After the law's enactment, Mexican FBNC youth experience a 63 percent decline in their high-school-diploma-attainment likelihood (Model 1 OR: 0.37, $p < 0.000$). This translates into these youth being three times less likely to have a high-school diploma than before. At the same time, there is no significant difference in high-school diploma attainment among citizens of Mexican ancestry across IIRIRA ($p = 0.247$).

To test the robustness of this result to the covariates I described earlier, following Potochnick (2014), I add time-varying individual characteristics: age, sex, and marital

[14] A baseline difference-in-differences model regresses the policy outcome on a policy-effect interaction term and dummy variables for the pre-policy treatment-group and post-policy control group.

status to the baseline model in Model 2, and reach a similar finding (OR: 0.31, $p < 0.000$). In Model 3, I add to the baseline model: 1) monthly unemployment rate to account for state-specific economic trends; 2) the percentage of white non-Latino adults ages 30-45 who report having at least some college education to control for state-specific education trends; 3) the percentage of Mexican adults of the same age cohort who report having a high-school diploma to account for community educational attainment in the absence of parental education measures; and 4) the percentage of persons in a state who report being Mexican FBNCs in a given month to measure state migration trends. These results are robust to the inclusion of these time-varying state characteristics (OR: 0.39, $p < 0.000$), as well as to the inclusion in Model 4 of both individual and state time-varying characteristics (OR: 0.31, $p < 0.000$).

In Model 5, I add state fixed effects to Model 4 to account for otherwise unobserved fixed differences between the states, such as differences in laws and policies affecting undocumented youth, enactment and enforcement of anti-affirmative-action laws, and social and community services available to this population; year fixed effects to account for otherwise unobserved national-trend differences associated with time, such as differences in immigration enforcement and education regulations that applied uniformly across states; and month fixed effects to account for otherwise unmeasured differences in educational attainment by month, with an expectation of spikes in high-school diploma attainment between May and July when many high-school seniors across the nation graduate. My finding remains robust to the simultaneous inclusion of time-varying and time-invariant characteristics (OR: 0.33, $p <0.001$). To weigh against my results being confounded by unobserved state trends that, apart from fixed state and demographic

characteristic and time-varying observables, might confound my findings, I add state-by-year fixed effects in my final Model 6. My findings are stable in terms of both effect size and significance across all six models (Model 6 OR: 0.37, $p < 0.001$). According to my final-model estimate, FBNC youth are slightly more than two-and-a-half times less likely to have a bachelor's degree than a citizen youth before IIRIRA

As I show in Appendix 2, these findings are stable in both magnitude and statistical significance to the different groupings of states (see cf. Flores and Chapa 2008; Potochnick 2014): in Model 7 to the 10 states where Mexican immigrants traditionally settle: Arizona, California, Colorado, Florida, Illinois, Nevada, New Jersey, New York, or Texas (OR: 0.37 $p < 0.001$); in Model 8 to the 10 states with the highest population percentages of Mexican immigrants in 1996, when IIRIRA was passed: California, Colorado, Florida, Idaho, Illinois, Nevada, New York, Oregon, Texas, and Utah (OR: 0.34, $p < 0.001$); and in Model 9 to the 10 states which passed in-state-resident-tuition (ISRT) laws within 10 years of IIRIRA's passage: California, Illinois, Kansas, Nebraska, New Mexico, New York, Oklahoma, Texas, Utah, and Washington (OR: 0.44, $p <0.001$). I conduct two additional sets of analyses to explore whether my observed no-effect is spurious and as a further robustness check on my identification of Mexican FBNC as likely undocumented students. First, I evaluated whether IIRIRA affected non-Latino FBNC and non-Latino citizens, and found no effect on either group (Model 10 (Latino FBNC): $p = 0.390$; Model 11 (non-Latino Citizens): $p = 0.978$). These findings suggest that I have isolated the population most likely to be impacted by IIRIRA by proxying Mexican FBNC for likely undocumented youth.

One possible explanation for my findings could be the influence of anticipatory shocks, either due to publicity surrounding IIRIRA's passage or the enactment of another law which might have influenced high-school diploma attainment adversely before IIRIRA became effective. I test this in Model 12 by analyzing this outcome across IIRIRA's September 30, 1996 passage date (Model 12 OR: 0.26, $p < 0.001$), and across PRWORA's enactment date, which was one year prior to IIRIRA's enactment date on July 1, 1997 (Model 13 OR: 0.22, $p < 0.001$), and find substantial and significant findings. The most plausible explanation for these findings is that FBNC diploma attainment was on an upward trajectory at the time of IIRIRA's passage, at least until PRWORA was enacted nine months later. It is possible that the *passage* of IIRIRA dampened or otherwise arrested the gains undocumented youth might have made in high-school diploma attainment above and beyond the impact the law had once enacted.

Taken together, these results support a conclusion that IIRIRA caused a reduction in undocumented youth's high-school diploma attainment odds. The immediacy and the magnitude of this decline suggests, against prior research, that FERPA, *Plyler*, and the plethora of policies and practices districts and states have set in place to preserve undocumented youth's access to a K-12 education do not work in the face of such severe, restrictive immigration reform. Given IIRIRA's nature as a comprehensive law, there are a number of possible explanations for this outcome.

One possible explanation suggests that IIRIRA decreased the benefit of a high-school education by making it harder for an undocumented young adult to seek gainful, lawful employment. In addition to provisions restricting higher-education benefits, IIRIRA also changed a number of workplace verification laws. Every employer is now

required to verify the identity and right to work of each employee it hired after September 30, 1996 (see 8 U.S.C. § 1324(a)(1996). For his or her part, each would-be employee has to submit either a U.S. passport, alien registration card, or a combination of a social security card and a verified photo identification card to verify right to work (§ 1324(a)). These were substantial changes to previous law. Before IIRIRA, employers could not specify which documents an employee had to present to verify his or her right to work. Such action constituted unlawful discrimination (*Jones v. DeWitt Nursery Home* (1990) in Osuna 1997). Many non-U.S. nationals and undocumented workers presented foreign passports or birth certificates, which before IIRIRA were acceptable authorizing documents (see 8 U.S.C. § 1324(a)(1996)). After IIRIRA, employers were also given greater discretion to demand "more or different documents" verifying right to work, and could be found to have engaged in unlawful discrimination—even on racial or ethnic grounds—only if he or she had the intent to do so (§ 1324; Osuna 1997).

In his ethnography of undocumented youth coming of age, Gonzales (2016) finds not only that educational attainment matters far less than undocumented immigration status for these youth, but that persisting to a high-school diploma might make it *harder* for undocumented young people to enter into the clandestine labor market available to them—the same market available to their parents who tend to have less formal education and English-language fluency (Abrego and Gonzales 2010). Paradoxically, by remaining in school, undocumented youth forego accumulating job experience that makes them competitive for scarce job positions and might insulate them from early termination. Gonzales also suggests inchoately that with longer time in formal school settings comes a greater expectation for fair wages, safe working conditions, and fair treatment,

expectations that ultimately do them a disservice. Perversely, by making it harder to enter into the legitimate work force, IIRIRA appears to have improved the wage premium for likely undocumented youth on exiting from high school before receiving a diploma.

Another perverse incentive which might explain such high declines in diploma attainment is non-compliance with compulsory school attendance laws. Citizen children of undocumented parents are known to be more likely to dropout than children of legal resident and citizen parents in part because of lower levels of engagement by these parents in their children's education (cf. Tienda and Haskins 2011). Though writing on the impacts to younger children's education, Yoshikawa's (2011) work uncovering the role of residency-status stress on academic attainment is illuminating in this regard. He finds that undocumented parents are generally reluctant to engage with educational institutions—institutions with which they must engage due to compulsory school attendance laws—out of fear of discovery and deportation. IIRIRA's passage is also known to have increased the fear of deportation among undocumented families with school-age children, despite INS guidelines which discourage detaining individuals on school property (Olivos and Mendoza 2010; but see Gonzales, Heredia and Negrón-Gonzales 2015). These fears were not without merit. Though Congressional Research Service data show a steady rise in INS detentions in the years before IIRIRA was passed, it shows an exponential increase between 1997 and 1998 when IIRIRA became fully enforceable (Siskin 2004), the largest between-year rate increase in detentions since IRCA (see Siskin 2012). It is not beyond reason to impute deportation fears onto youth whom are undocumented themselves, and extend such fears to explaining an IIRIRA-related decline attendance and in high-school diploma attainment consequently.

Gender and marital status separately, and perhaps together, might explain differences in IIRIRA's effect on diploma attainment. I evaluate gender heterogeneous effects by modeling policy effects for men separately from women in Models 14 and 15 respectively. While Mexican FBNC women are twice as unlikely as citizen women to have a high-school diploma after IIRIRA (Model 15 OR: 0.46, $p < 0.001$), Mexican FBNC men are three times as unlikely (Model 14 OR: 0.32, $p < 0.001$).[15] This result is somewhat consistent with research indicating a gender differential in high-school diploma attainment among Latinos broadly (see, e.g., Murnane, 2013).

Marriage appears to play a significant and different role in diploma attainment likelihoods by gender. Mexican FBNC married women are four times less likely to have a high-school diploma after IIRIRA (Model 16 OR: 0.26, $p = 0.013$), while unmarried Mexican FBNC women are two-and-a-half times less likely (Model 17 OR: 0.38, $p < 0.001$). Compared to Mexican FBNC married women, Mexican FBNC men are 25 percent more likely to have a high-school diploma after IIRIRA (Model 16 OR (Male): 1.25, $p < 0.001$).

Relatively speaking, while an unmarried Mexican FBNC man is 45 percent more likely to have a high-school diploma after than a married Mexican FBNC man (Model 14 OR (Married): 0.56, $p < 0.001$), he is 62 percent *less* likely than an unmarried Mexican FBNC woman (Model 17 OR (Male): 0.62, $p < 0.001$). For her part, an unmarried Mexican FBNC woman is 69 percent more likely to have a high-school diploma than a married Mexican woman (Model 15 OR (Married): 0.31, $p < 0.001$). These results are also consistent with prior research (e.g., Espinoza 2010; Rosas and Hamrick 2002; Sy and

[15] Ideally, I would have liked to evaluate whether this gender difference in effect were statistically significant in a triple-difference (DDD) model. However, computational difficulties made specifying DDD models infeasible.

Romero 2008), but the causal direction is unclear and a differential analyses of possible explanations does not weigh in favor of one over any other.

On the one hand, as the aforementioned studies suggest, married likely-undocumented women may have more formal responsibilities to the household in addition to societal and cultural ones, and these might lead to earlier abandonment of academic pursuits (see also C. Suárez-Orozco et al., 2015). On the other hand, an early exiting undocumented woman might be more likely to marry at a younger age, which could explain these results. Then again, a likely-undocumented married man who had exited early from high school might pursue a GED in hopes of improving his job prospects and his family's circumstances. This would not be unlike the phenomenon observed among married citizen men who are more likely than their peers to return to college after delay (cf. Teachman and Polonko 1988). Whatever the cause, these results are alarming, even more so because these are the comparative odds of Mexican FBNC high-school-diploma attainment against citizen youth of Mexican ancestry, who against the rest of the high-school population have startlingly high dropout rates (Abrego and Gonzales 2010).

2. College Enrollment

<INSERT TABLE 3 HERE>

IIRIRA's effect does not appear at first glance to have extended to college enrollment. As I show in Model 1 on Table 3, without covariate adjustment, a Mexican FBNC youth between the ages of 18 and 24 appears to be half as likely as his U.S.-citizen co-ethnic peer to enroll in college before IIRIRA (OR: 0.50, $p = 0.031$). While there is no significant difference in college enrollment odds among citizens of Mexican ancestry

across IIRIRA (p = 0.161), my baseline results show an impact for FBNC youth (Model 1 OR: 0.42, p = 0.011). This finding is robust to the addition of time-varying individual- (Model 2 OR: 0.57, p = 0.029) and state-level characteristics (Model 3 OR: 0.48, p = 0.013), but including both sets of time-varying characteristics both lowers the difference in odds and marginalizes the statistical significance (Model 4 OR: 0.67, p = 0.064). With the addition of state, year, and month fixed effects, the coefficient becomes insignificant (Model 5 OR: 0.90, p = 0.696). Upon the inclusion of state-by-year fixed effects, the coefficient reverses direction (Model 6 OR: 1.03, p = 0.919).

My no-effect finding remains persistent when I limit my analytic sample to specific groupings of states, which I show in Appendix 3: in Model 7 to the traditional immigrant destination states (p = 0.960), in Model 8 to states with high concentrations of Mexican immigrants in 1996 (p = 0.926), and in Model 9 to early-passing ISRT states (p = 0.657). I also find no evidence of an IIRIRA event on non-Latino FBNC (Model 10: p = 0.424) or non-Latino citizens (Model 11: p = 0.815). I also do not find evidence of an earlier shock, either at IIRIRA's passage date (Model 12: p = 0.195) or at PRWORA's enactment date (Model 13: p =0.209). Overall, these findings clearly support a no-effect finding on college enrollment.

However, the strength of a finding on high-school enrollment coupled with a no-effect finding here might suggest unobserved heterogeneity early exiting undocumented youth and those who persist to enroll in college. A key predictor of high-school underperformance and by extension early exiting and non-persistence to college enrollment is residency status (Olivos and Mendoza 2010). For students who become aware of their residency status early in high school, or earlier, the subsequent stress,

depression, and perception of a conscribed future manifests itself through lower classroom performance, increased disciplinary problems, and eventual withdrawal from school (Abrego and Gonzales, 2010; Gonzales 2011; 2016). Inversely, students who become aware of their status later in their high-school careers closer to college enrollment might exhibit fewer outward manifestations that would impact their academic records, and consequently the willingness of an important cadre of school counselors, community and philanthropic funders, and college officials to help the successfully transition to college.

Gonzales (2010) finds that K-12 institutions structure academic opportunities and access to critical information about how to apply for and fund college to limit the number of undocumented students that are able to go on to higher education. Conversely, it appears, the undocumented students who do manage to make the transition are highly achieving, and are more likely to benefit from a reinforcing narrative of meritocracy that, in the absence of enforceable rights to college access, becomes physically embodied in the whimsical decision making of bureaucrats—both in high-school and in college—who decide whom to assist and to what degree (cf. Abrego 2008; Enriquez 2009; Contreras 2009; Gonzales 2009; Perez et al. 2010; C. Suárez-Orozco et al. 2015).

An alternate, but not inconsistent explanation for this no-effect finding might be unobserved similarities between likely undocumented and citizen youth of Mexican ancestry. Undocumented youth are likely to have attended the same under-resourced schools that poorly prepare high-school graduates for college success (Gonzales 2016). Those that do go to college, like their citizen peers, are likely to be first-generation college goers, and as such face the same challenges to accessing enrollment information

(Suárez-Orozco et al. 2015) and adjusting to hostile campus environments (Jack 2016). First-generation citizen youth are often unaware of the same federal financial aid funding their undocumented peers are ineligible to take advantage of (De La Rosa 2006), with the same net result (Collier and Morgan 2008). Both citizen and undocumented first-generation students report higher levels of working, both to fund their education and contribute to their families (compare Contreras 2009, and Gonzales 2016 (undocumented students), with De La Rosa and Tierney 2006 (Latino youth generally); and lower levels of engagement with faculty and staff, the sum of which yields similarly low levels of awareness of campus resources needed to thrive and persist to graduation (Perez et al. 2010).

3. Associate's Degree Attainment

<INSERT TABLE 4 HERE>

Despite not-readily-apparent effects on college enrollment, the enactment on IIRIRA did lead to declines in associate's degree attainment among Mexican FBNC adults between the ages of 22 and 30. In Table 4 I show that before IIRIRA, FBNC youth were three times less likely than their citizen peers to hold an associate's degree or higher (OR: 0.33, $p < 0.001$). I also show that after IIRIRA, U.S. citizen youth of Mexican ancestry were 49 percent *more* likely than their before-IIRIRA counterparts to hold such degrees (OR: 1.49, $p < 0.001$). I estimate, as a baseline measure, that the already low associate's degree odds of FBNC youth dropped three fold after IIRIRA went into effect (OR: 0.32, $p < 0.001$). As I control for time-varying individual characteristics (Model 2 OR: 0.32, $p < 0.001$), time-varying state characteristics (Model 3 OR: 0.29, $p = 0.001$), both sets of time-varying characteristics (Model 4 OR: 0.28, $p < 0.001$), all covariates and

state, year, and month fixed effects (Model 5 OR: 0.22: p = 0.002), and state-by-year fixed effects (Model 6 OR: 0.22, p < 0.001), the estimated effect of IIRIRA on associate's-degree holding increases in magnitude. According to my final-model estimate, FBNC youth are approximately *four times less likely* to hold an associate's degree than a citizen youth before IIRIRA. At the same time, I observe no covariate-adjusted difference in citizen associate's-degree odds across IIRIRA (p = 0.981).

As I show in Appendix 4, my effect finding is robust to limiting my analyses to traditional immigration states (Model 7 OR: 0.24, p = 0.002), states with high concentrations of Mexican immigrants in 1996 (Model 8 OR: 0.26, p = 0.003), and early-passing ISRT states (Model 9 OR: 0.30, p = 0.002). While I find no IIRIRA spillover associate's-degree effect on non-Latino FBNC (Model 10: p = 0.438), I find a significant effect in the *opposite direction* for non-Latino citizens (Model 11 OR: 1.76, p = 0.001). In addition to supporting a causal-effect finding for Mexican FBNC youth, the sum of these falsification findings offers evidence that might confirm policy proponents' stated justification for including tuition and funding restrictions in IIRIRA. I do find evidence of earlier shocks at IIRIRA's passage (Model 12 OR: 0.18, p < 0.001) and at PRWORA's passage (Model 13 OR: 0.21, p < 0.001). This is not surprising given the near-simultaneous passage of PRWORA and IIRIRA, and the general shock PRWORA is known to have had on the broader undocumented community, and specifically to college students with the elimination of federal financial aid eligibility. Given these observations, it is more plausible that I observe in these falsification tests evidence of multiple, compounding policy signals rather than a spurious effect on associate's degree holding.

These results support the conclusion that IIRIRA's enactment caused a dramatic reduction in likely undocumented young adults' already low rates of associate's-degree attainment. Studies consistently show that undocumented youth who persist to college enrollment are high academic performers, highly motivated, resourceful, and determined to complete a degree (e.g., Contreras 2009; Gonzales 2016; Perez et al. 2010; C. Suárez-Orozco et al. 2015; Terriquez 2015). Their non-persistence to an associate's degree is not likely a result of their inability or lack of desire to do so. Given this, and in the absence of a finding of reduced enrollment odds, my finding suggests that factors unique to undocumented young people operate between enrollment and graduation to deny them degree attainment.

In their ecological analysis of a national survey of undocumented students attending both two- and four-year colleges, Carola Suárez-Orozco et al. (2015) report that 95 percent of community college students experience anxiety about their ability to afford college, with 29 percent experiencing extreme concerns. The primary barrier to undocumented students obtaining a college degree is cost (Baum, Ma and Payea 2013). Even though many choose to enroll in community college, live at home, and go without books and other necessary supplies, the cost of college over time is usually overwhelming. In testimony before Congress, Long (2013) revealed that the cost of higher education is prohibitive for many students—who have access to federal financial aid. Most undocumented students cannot rely on their families to help them pay for college. Fifty-four percent of community-college students in Suárez-Orozco et al.'s (2015) study report paying for at least half their tuition out-of-pocket.

Many students facing recurring costs "stop out" of college (Terriquez 2015). "Stopping out" refers to a temporary withdrawal from college with the intent to return to complete a degree (Terriquez). Among undocumented students who do complete their degrees, their college enrollment history tends to reflect a patchwork of half-time enrollments financed by numerous odds-and-ends jobs these students often fit around their school schedules followed by non-enrollment periods dedicated to working and raising tuition costs (Gonzales 2016; see also Suárez-Orozco et al. 2015). Over time, the stress and physical toll of this process compound with limited progress toward degree completion leading most undocumented students who stop out never to return (Gonzales 2016; Terriquez 2015). While this phenomenon is not unique to undocumented students, without either meaningful salaries or financial aid to fund their college pursuits, undocumented students disproportionately stop out compared to their citizen peers (Terriquez 2015).

In addition to financial cost, undocumented college students report greater feelings of social and academic isolation than their peers (Gonzales 2016; C. Suárez-Orozco et al., 2015). In the face of unfriendly, if not unavailable, student supports, unless they have affirming peer networks and substantial community support, the psychological costs of feeling like an outsider often weighs against persistence (cf. Perez 2010). Undocumented students are also less likely than their citizen peers to ask for academic assistance or engage with professors with whom they do not have a personal trust relationship (C. Suárez-Orozco et al. 2015). These ever present stresses also leave little time for academic work, impacting performance, which, unsurprisingly often leads to a

lack of return on costly investments in the form of no credit for failing grades (Gonzales 2016).

Consistent with prior research, women, particularly married women, appear to shoulder a greater burden of IIRIRA's effect on associate's-degree attainment (Espinoza 2010; Rosas and Hamrick 2002; Sy and Romero 2008; but see Jacobs and King 2002 (finding that female undergraduates 25 and older were more likely than their younger peers to be married). Mexican FBNC women are approximately five times less likely than their citizen counterparts to have an associate's degree after IIRIRA (Model 15 OR: 0.19, $p < 0.001$). Being married reduces cuts these odds in half (Model 15 (Married) OR: 0.69, $p < 0.001$). While there is no statistically significant difference in the odds of married persons (Model 16 OR: 0.20, $p < 0.001$) by gender (Model 16 (Male) OR $p = 0.132$), unmarried *men* are 38 percent less likely (Model 17 (Male) OR: 0.73, $p < 0.001$) to hold an associate's degree than unmarried women (Model 17 OR: 0.23, $p < 0.001$), whose odds more closely mirror the general trend (Model 6 OR: 0.22, $p < 0.001$). As a result, married women and unmarried men appear to have similarly lower odds of degree attainment. This similarity in effect is not surprising given Teachman and Polonko's (1988) finding that being married encourages men who delayed enrollment to start and complete a degree.

4. Bachelor's Degree Attainment

<INSERT TABLE 5 HERE>

Unsurprisingly, as I show in Table 5, I find a more substantial effect when I restrict the educational attainment outcome to FBNC youth who have earned a bachelor's degree or higher. In my baseline Model 1, I show that FBNC youth were two-and-a-half

times less likely than their citizen peers to hold a bachelor's degree or higher before IIRIRA (OR: 0.37, $p < 0.001$). While after IIRIRA, citizen youth were 43 percent more likely than before to hold a bachelor's degree (OR: 1.43, $p = 0.001$), FBNC youth were an additional two-and-a-half times less so (OR: 0.35.3, $p < 0.001$), resulting in a comparative odds ratio of 0.13, which translates to a *seven times lesser likelihood* of an FBNC youth after IIRIRA holding a bachelor's degree when compared to his citizen counterpart before the law ($p < 0.000$).

As expected, given we know that the majority of college enrollees and degree holders among the undocumented do so at the community college level and receive associate's degrees (P. Perez 2010), removing associate's degrees from the count before assessing degree odds exacerbates degree-attainment disparities. My baseline findings from Model 1, as with associate's degrees, are stable to the inclusion of time-varying individual characteristics (Model 2 OR: 0.35, $p < 0.001$), time-varying state characteristics (Model 3 OR: 0.32, $p < 0.001$), both sets of time-varying characteristics (Model 4 OR: 0.31 $p < 0.001$), and the inclusion of all covariates and state, year, and month fixed effects (Model 5 OR: 0.23, $p < 0.001$).

According to my covariate-adjusted estimate, which also includes state-by-year fixed effects, Mexican FBNC youth are approximately five times less likely to hold a bachelor's degree after IIRIRA than a citizen youth before IIRIRA (OR: 0.21, $p < 0.001$). Stated differently, for every 100 likely undocumented young people who held a bachelor's degree before IIRIRA, only 21 did after the law was enacted.

My bachelor's-degree-attainment effects are generally robust to the states being analyzed. These low odds persist whether examining bachelor's-degree holding among

Mexican FBNC living in traditional immigration states (Model 6 OR: 0.22, $p < 0.001$), states with high concentrations of Mexican immigrants in 1996 (Model 7 OR: 0.26, $p < 0.001$), or early-passing ISRT states (Model 8 OR: 0.31, $p < 0.001$). I find substantial, significant counter effects of IIRIRA for non-Latino FBNC (Model 10 OR: 1.63, $p = 0.021$) or citizens (Model 11 OR: 2.13, $p < 0.001$). These findings suggest that IIRIRA had a suppressive effect on likely undocumented youth contemporaneous with the intended beneficial effect on other groups. As with my associate's degree findings, I find unsurprising evidence of earlier shocks at IIRIRA's passage (Model 12 OR: 0.22, $p < 0.001$), and a marginally significant effect at PRWORA's passage (Model 13 OR: 0.21, $p < 0.001$).

I observe a more substantial effect for Mexican FBNC men when limiting educational attainment to bachelor's-degree holders (Model 14 OR: 0.18, $p < 0.001$). Likely undocumented men, in particular, appear to be six times less likely to have a bachelor's degree than their citizen peers. The IIRIRA effect on Mexican FBNC women's bachelor's-degree odds more closely parallels the general effect (Model 15 OR: 0.21, $p < 0.001$). Married Mexican FBNC women, however, appear to the same odds of degree attainment when compared to married citizen women (Model 16 OR: 0.20, $p < 0.001$) as their unmarried cohort in comparison to unmarried citizen women (Model 17 OR: 0.20, $p < 0.001$).

In 1995, one year before Congress passed IIRIRA, Tyler, Murnane, and Levy found that an associate's degree had as much market value as a high-school diploma did a generation prior. Fifteen years later, Deming and Dynarksi (2010) found the *bachelor's*

degree to be this generation's high-school-diploma equivalent with respect to earning power and professional returns.

Most undocumented students who wish to pursue a bachelor's degree enroll first in community college to take advantage of the lower tuition in two-year schools (Perez 2010). Only about one in five students who enter higher education through community college transfers successfully to a four-year college (see Gonzales 2016). Though my enrollment results cannot speak to this directly, evidence suggests that undocumented community-college students face harsher odds. It is axiomatic that if community college costs overwhelm most undocumented students in the absence of financial aid, then the compounding costs of pursuing a four-year degree at more expensive rates makes bachelor-degree attainment practically prohibitive (compare Baum, Ma and Payea 2013, with Passel and Taylor 2010).

Those students who manage to make the transition, and those who begin their higher education at four-year colleges, are more likely to do so under a convergence of unusually favorable circumstances. Financial sponsorship, usually from private philanthropic organizations, but occasionally from a community network, is a key predictor in likelihood of persistence to graduation (Gonzales 2010). These students are more likely than not to have strong network ties to other undocumented students and other allies and supporters (Perez et al. 2010; C. Suárez-Orozco et al. 2015; Gonzales 2016). Their families also sacrifice, most profoundly in terms of reduced or lost wages (Gonzales 2016).

VI. Implications and Conclusion

As undocumented youth mature into adulthood, their lives become increasingly regulated by IIRIRA. By denying these young Americans authorization to work and licenses, IIRIRA limits their ability to enter into the legitimate work force and travel freely. The imposition of this legal disability leads to a series of compounding, often hidden, economic and social disabilities with grave consequences, not only for the approximately 1.1 million undocumented youth under the age of 18 (Passel and Cohn 2015), but also for their families and communities as they age. Higher education is one of the few activities undocumented Americans are allowed to pursue under the law. Yet, for most undocumented young people, a bachelor's degree remains an impossible dream.

In this paper, I explore whether IIRIRA contributes to this phenomenon by examining whether rates of high-school completion, college enrollment, and associate's- and bachelor's-degree attainment changed after the law's enactment. Using Mexican foreign-born non-citizens as a proxy for being likely undocumented, I find dramatic, immediate declines in high-school diploma attainment among likely undocumented youth ages 18-21 and associate's- and bachelor's-degree attainment among likely undocumented youth ages 22-30, when comparing these youth to their U.S. citizen and authorized resident counterparts. I conclude, thus, that despite being allowed to attend college, undocumented Americans, on balance, do not attain a college education in measureable part due to IIRIRA.

These findings are consistent with research documenting how, as a consequence of the childhood suspension of "illegality" being sharply discontinued due to IIRIRA, undocumented youth on the threshold of adulthood face rapidly narrowing opportunities

for education and work and mounting costs associated with their pursuit. With each passing stage along the educational trajectory, fewer undocumented students achieve as "illegality" transforms into a master status that dominates young people's existence. A college education is frankly worth less to an undocumented young adult who cannot use it to secure the same economic success and social mobility his citizen and authorized resident peers can. These observations are important for understanding the accruing cost to a group of young Americans in danger of becoming the permanent underclass the *Plyler* court sought to avoid. As such, they provide evidence to bear on how uniquely impactful federal immigration law is on the lives of these young people and the communities of which they are apart. They also provides an empirical basis for shift the conversation about undocumented students' college access toward a richer discussion of how law creates uniquely burdensome conditions for these youth at key transitional moments in late adolescence that are different from their U.S.-born and foreign-born peers, and how law might or might not ameliorate the same.

A variety of policy interventions at both the federal and state levels have proved incomplete. Much of the advocacy among undocumented rights supporters has focused on passing at the federal level various versions of the Development, Relief, and Education for Alien Minors Act, commonly called the "DREAM Act" (e.g., Olivas 2009; 2011). In 2001, U.S. Congressman Luis Gutierrez of Illinois and U.S. Senator Orrin Hatch from Utah introduced separate DREAM Act bills in their respective houses of Congress that would have amended IIRIRA to protect undocumented youth who had lived in the U.S. since early childhood from deportation and offer them a path to authorized residency and U.S. citizenship (H.R. 1582 2001; S. 1291 2001). Neither bill

passed. In each Congress since similar DREAM Act bills have been introduced in both houses with similar results. The DREAM Act came closest to passing Congress in November 2010, when the Senate was five votes short of passing H.R. 5281, which had previously passed the House of Representatives. Though imperfect remedies, all iterations of the DREAM Act would have allowed undocumented students to participate in federal loan and work-study financial-aid programs (Olivas 2009; H.R. 5281 2010).

In response to the DREAM Act's repeated failure to pass Congress, the Obama Administration introduced the Deferred Action for Childhood Arrivals (DACA) program in June 2012. DACA does not extend financial-aid eligibility to undocumented students. Rather, DACA offers temporary work authorization and assurance against deportation as primary relief for some putatively DREAM Act-eligible undocumented Americans (Department of Homeland Security 2012). A large percentage of DACA recipients report having obtained driver's licenses (61 percent), new jobs (61 percent), and open bank accounts (54 percent)--activities which were barred them before DACA (Gonzales and Terriquez 2013). Yet, of the nearly 1.7 million undocumented Americans believed to be DACA eligible (~ 15 percent of all undocumented Americans), only half have applied for deferment (Passel and Lopez 2012). Many DACA-eligible Americans have not applied for deferment because of burdensome administrative requirements, including the high $465 application fee (Gonzales and Terriquez 2013). Many also fear consequences of disclosing their immigration status to the federal government; concerns which are enhanced by DACA being an administrative action easily changed by presidential administrations or the courts (see, e.g., *U.S. v. Texas*, pending (the State of Texas challenging DACA as an unconstitutional administration of IIRIRA)).

Because of residency, education, and age requirements, among others, Wong and Valdivia (2014) note that many undocumented families contain both DACA-eligible and DACA-ineligible members, and that many eligible family members decline to apply for DACA out of solidarity with their ineligible family members. The Obama Administration sought to remedy this by extending deferment eligibility to many previously ineligible family members through the Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA) and extensions of DACA. An estimated 3.9 million undocumented Americans would be newly eligible for deferment under DAPA, and an additional 1.5 million newly eligible under the expanded DAPA, which would improve the percentage of deferment eligible individuals from 15 to 62 percent (Warren 2014). However, as stated above, administrative actions like DACA and DAPA are particularly vulnerable to changes in administration, congressional action, and judicial interpretations of the scope of IIRIRA. Indeed, implementation of DAPA and the extended DACA has been enjoined since the State of Texas and others sued the Obama Administration in 2014 (*Texas v. U.S.*, 2015). The states claims that both DAPA and DACA are improper administrations of immigration laws that restrict undocumented Americans' access to work, travel, and economic opportunities are currently before the U.S. Supreme Court (*U.S. v. Texas*, pending).

As federal administrative remedies have stalled, 17 states (as of 2013) have also intervened by passing ISRT laws seeking to mitigate the IIRIRA's impact on undocumented youth's educational attainment. Most of these laws only address disincentives to higher education posed by higher out-of-state tuition costs. While a handful include state-financial-aid benefits, these benefits are in no way as robust or as

uniformly available as the federal financial aid benefits upon which most college students rely to help meet growing tuition costs (Baum et al. 2013). Because IIRIRA is a federal law, the states alone cannot act to make this aid available to undocumented students. Their "illegality," per the law, remains a barrier to higher education. It also remains a barrier to work, and as a consequence limits the value of higher education—and even a high-school education—for undocumented young people who might otherwise pursue it. To the extent policymakers might want to improve the circumstances in which many undocumented families live, my findings imply that measures that do not amend IIRIRA to allow undocumented students' participation in the federal financial aid system and to pursue lawful employment might not make any meaningful difference.

My paper also better frames an understanding of the context in which ISRT policies work. Without this context, studies on ISRT's effectiveness present a more favorable picture on improving diploma and degree attainment than many undocumented students themselves experience. In the following paper I explore the role of higher out-of-state tuition costs by examining high-school graduation, college-enrollment, and degree-attainment outcomes among likely undocumented youth living in states that have passed IIRIRA-compliant in-state-resident-tuition (ISRT) laws to outcomes among similar youth who have not. Future research might examine the role of alternate sources of credit and funding in undocumented students' college access in the absence of federal financial aid.

It is quite plausible that policy alone is an insufficient solution for improving undocumented student access to college. Perhaps law provides the only comprehensive solution. I propose one such legal solution is the extension of *Plyler v. Doe* (1982) in one of two complementary ways: (1) acknowledge that a post-secondary education has

replaced a high-school education as the "basic education" spoken of in *Plyler*; or (2) establish education access as a fundamental right.

Recall that *Plyler* requires states to show that "deny[ing] a discrete group of innocent children the free public education that it offers to other children…furthers some substantial state interest." (1982: 230). Concerned with "impos[ing] a lifetime hardship…[and] illiteracy," the Court understood that:

> By denying these children a basic education, we deny them the ability to live within the structure of our civic institutions, and foreclose any realistic possibility that they will contribute in even the smallest way to the progress of our Nation

(224). A basic education in the early 1980s was commonly understood to be a high-school education—an education sufficient for one to enter into the workforce and take care of his or her family. The functional equivalent of the early-1980s high-school education is generally understood in 2016 to be a bachelor's degree (Deming and Dynarski 2010). Given this, it follows that by removing access to federal financial aid and largely requiring states to charge undocumented students higher tuition through IIRIRA, the 104th Congress did precisely what the 1975 Texas legislature did in 1975, and with similarly unjustifiable interests.

Two items complicate this analysis. One, unlike a public K-12 education, a college education is not free. Two, the federal government enjoys plenary power of immigration that the states do not (cf. *Zadyvdas v. Davis* 2001). To the first, the *Plyler* Court was not moved by the absence of cost in K-12 education as much as it was the equivalency of cost for all in-state residents. The Court specifically found that undocumented students are "basically indistinguishable" from authorized-resident and citizen students in terms of educational cost and need, rendering the cost justification inapposite (*Plyler* 1982:229). To the second, under current jurisprudence education rights

are understood as determined by states (*Goss v. Lopez* 1975). As Justice Blackmun noted in his concurrence, it is improper for states to allocate *residence-based rights* based on federal *immigration status* (*Plyler* 1982: 236 (Blackmun, J., concurring)).

Because the federal government has the rights to regulate non-citizens within its borders (cf. *Zadyvdas* 2001) and to spend for the general welfare (U.S. CONST. ART. I, § 8, ¶1), simply extending *Plyler* according to its current terms could not require the federal government to extend federal financial aid to undocumented students. This would require a more ambitious extension, an elevation of education, or at least education access, as a fundamental right. While many believe the question is foreclosed by *San Antonio v. Rodriguez* (1973), I submit *Papasan v. Allain* (1986) as evidence that the Supreme Court itself has left the question open:

> As *Rodriguez* and *Plyler* indicate, this Court has not yet definitively settled the questions whether a minimally adequate education is a fundamental right and whether a statute alleged to discriminatorily infringe that right should be accorded heightened equal protection review.

While *Papasan*, a case involving unequal funding in Mississippi for Chickasaw Nation schools, did not allege that Chickasaw students were not have access to a basic education, one might argue through carefully curated cases, given the mounting evidence that positions a bachelor's degree as an essential educational accomplishment, that IIRIRA discriminatorily infringes on undocumented students' educational access rights. Because the federal government has the same equal protection responsibilities as states (*Bolling v. Sharpe* 1954), IIRIRA provisions that are proved to infringe on education access rights might violate the Fifth Amendment. Above and beyond its benefit to undocumented students, a move toward securing education, or any discreet portion thereof, such as

access, as a federally recognized right would have incredible implications for reducing educational inequalities.

Figure 1. Timeline of significant events in undocumented-student college access.

Year	Facilitative Event	Restrictive Event
1982	*Plyler v. Doe*: States are required to allow undocumented children access to public K-12 education	
August 22, 1996		Congress passes Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA); President Clinton signs.
September 30, 1996		Congress passes Immigrant Reform and Immigrant Responsibility Act of 1996 (IIRIRA); President Clinton signs.
July 1, 1997		PRWORA effective: Undocumented ineligible for federal financial aid
July 1, 1998		IIRIRA effective: States cannot offer in-state-resident tuition (ISRT) to undocumented unless same offer is made to citizens and legal residents. Higher Education Act amended: Colleges must submit proof of financial aid applicant's lawful immigration status to INS (USCIS).
1999		
2000		
June 16, 2001	Texas passes first state to pass in-state-resident-tuition (ISRT) law; TX ISRT law immediately effective	

Table 1. Summary Statistics.

	Foreign-Born Non-Citizen					U.S. Citizens				
	Before IIRIRA		After IIRIRA			Before IIRIRA		After IIRIRA		
	Mean	SE	Mean	SE		Mean	SE	Mean	SE	
Age 18-21										
Outcome										
High-School Diploma (%)	31.20	3.02	32.95	4.44		57.13	3.29	60.31	2.33	
Covariates										
Individual Characteristics										
Age (years)	19.77	0.03	19.71	0.10		19.40	0.07	19.45	3.08	
Male (%)	53.78	4.09	57.55	2.39		45.90	2.24	54.29	1.39	**
Married (%)	26.70	3.63	16.83	2.22	*	15.89	3.13	12.68	2.73	*
State Characteristics										
Unemployment Rate (%)	6.40	0.56	4.62	0.19	***	6.05	0.46	4.67	0.21	***
Mexican (%)	17.97	2.40	15.54	3.92		17.58	2.70	20.64	2.29	**
Mexican FBNC (%)	7.99	1.58	6.37	1.59	+	6.59	1.52	7.16	1.08	
White, HS Diploma (30-45, %)	53.47	1.65	52.47	1.40	*	52.90	1.63	51.81	1.36	***
# of observations	2587		2164			4899		6644		
Age 18-24										
Outcome										
Enrolled in College (%)	5.22	0.59	4.45	0.61	*	9.94	1.98	7.10	0.44	
Covariates										
Individual Characteristics										
Age (years)	21.54	0.05	21.41	0.09		21.00	0.15	20.77	2.38	
Male (%)	60.38	1.14	59.95	1.80		52.18	2.12	53.51	1.56	
Married (%)	38.14	1.80	30.51	2.57	*	22.65	2.31	21.50	4.15	
State Characteristics										

Unemployment Rate (%)	6.32	0.53	4.54	0.22	***	6.07	0.43	4.66	0.22	***
Mexican (%)	17.25	2.64	15.82	3.48		17.99	2.44	20.41	2.33	**
Mexican FBNC (%)	7.79	1.67	6.45	1.40	+	6.92	1.57	7.13	1.08	
White, some college (30-45, %)	53.24	1.72	52.10	1.41	+	52.68	1.54	51.93	1.42	*
# of observations	5586		4392			8604		6644		
Age 22-30										
Outcome										
Associate's Degree or higher (%)	3.96	0.26	3.81	0.30		11.14	0.64	15.77	1.38	***
Bachelor's Degree or higher (%)	2.42	0.21	2.33	0.28		6.31	0.56	7.79	0.85	*
Covariates										
Individual Characteristics										
Age (years)	26.04	0.10	26.12	0.14		25.63	0.27	25.93	0.05	
Male (%)	60.32	2.38	59.46	2.14		56.58	2.18	54.03	1.81	
Married (%)	60.41	1.62	60.64	3.28		45.79	3.68	48.85	0.76	
State Characteristics										
Unemployment Rate (%)	6.34	0.53	4.58	0.24	***	6.18	0.45	4.62	0.21	***
Mexican (%)	16.67	2.84	16.27	3.40		18.93	2.20	19.99	2.43	*
Mexican FBNC (%)	7.61	1.76	6.83	1.56	+	7.34	1.53	7.08	1.17	
White, some college (30-45, %)	53.32	1.64	52.28	1.63	+	52.92	1.66	52.54	1.31	
White, BA or higher (30-45, %)	22.89	0.27	19.36	0.69		18.55	0.72	19.16	0.65	+
# of observations	9503		7921			9830		7153		

Notes: Robust standard errors. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ indicate statistical significance for mean comparisons of before IIRIRA and after IIRIRA observations using adjusted Wald tests.

Table 2. Main Effects, High-School-Diploma Attainment.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ State, Year, Month FE	+ State×Year FE
FBNC after IIRIRA	0.369***	0.311***	0.396***	0.308***	0.330***	0.372***
(1 = yes; 0 = no)	(0.081)	(0.079)	(0.089)	(0.084)	(0.058)	(0.049)
FBNC	0.340***	0.302***	0.317***	0.281***	0.294***	0.262***
(1 = yes; 0 = no)	(0.062)	(0.055)	(0.060)	(0.051)	(0.053)	(0.037)
After IIRIRA	1.140	1.110	1.197+	1.070	0.952	1.062
(1 = yes; 0 = no)	(0.127)	(0.119)	(0.117)	(0.117)	(0.159)	(0.123)
Age (years)		1.548***		1.556***	1.544***	1.587***
		(0.078)		(0.070)	(0.042)	(0.026)
Male		0.793+		0.797+	0.743**	0.698***
(1 = yes; 0 = no)		(0.100)		(0.101)	(0.073)	(0.056)
Married		0.392**		0.398**	0.369***	0.371***
(1 = yes; 0 = no)		(0.112)		(0.114)	(0.097)	(0.091)
Unemployment Rate (%)			1.049	0.992	1.067	0.939
			(0.073)	(0.072)	(0.067)	(0.186)
30-45 Whites w/ some college			1.131	1.035	1.067	1.262
(1 = yes; 0 = no)			(0.306)	(0.284)	(0.331)	(0.408)
Mexican FBNC in a state (%)			1.033	1.044	0.965	0.971
			(0.028)	(0.031)	(0.042)	(0.036)
Constant	1.333*	0.000***	0.755	0.000***	0.000***	0.000***
	(0.179)	(0.000)	(0.260)	(0.000)	(0.000)	(0.001)
Observations	13884	13884	13884	13884	13877	13690

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses.

Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Table 3. Main Effects, College Enrollment.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ State, Year, Month FE	+ State×Year FE
FBNC after IIRIRA (1 = yes; 0 = no)	0.422*	0.570*	0.478*	0.674+	0.898	1.025
	(0.137)	(0.142)	(0.137)	(0.140)	(0.246)	(0.247)
FBNC (1 = yes; 0 = no)	0.499*	0.875	0.499*	0.884	1.002	1.086
	(0.157)	(0.208)	(0.149)	(0.197)	(0.204)	(0.192)
After IIRIRA (1 = yes; 0 = no)	0.693	0.567*	0.796	0.684*	0.937	1.102
	(0.179)	(0.120)	(0.149)	(0.101)	(0.218)	(0.187)
Age (years)		0.372***		0.370***	0.374***	0.377***
		(0.032)		(0.032)	(0.029)	(0.029)
Male (1 = yes; 0 = no)		0.856		0.861	0.944	0.967
		(0.155)		(0.153)	(0.121)	(0.128)
Married (1 = yes; 0 = no)		0.326***		0.325***	0.356***	0.353***
		(0.037)		(0.039)	(0.023)	(0.023)
Unemployment Rate (%)			1.094	1.121	0.833+	0.817
			(0.105)	(0.104)	(0.077)	(0.194)
30-45 Whites w/ some college (1 = yes; 0 = no)			1.618	1.288	1.475	1.332
			(0.501)	(0.368)	(0.419)	(0.377)
Mexican FBNC in a state (%)			0.969	0.968	1.005	0.999
			(0.047)	(0.037)	(0.007)	(0.017)
Constant	0.110***	3.67×10^{7}***	0.043***	2.14×10^{7}***	1.35×10^{8}***	1.22×10^{8}***
	(0.024)	(6.37×10^{7})	(0.038)	(3.26×10^{7})	(2.08×10^{8})	(1.67×10^{8})
Observations	25301	25301	25301	25301	25042	23524

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses.

Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 18-24 of Mexican ancestry. Values are presented in odds-ratios.

Table 4. Main Effects, Associate's-Degree Attainment.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ State, Year, Month FE	+ State×Year FE
FBNC after IIRIRA	0.316***	0.315***	0.289***	0.280***	0.223***	0.223***
(1 = yes; 0 = no)	(0.038)	(0.036)	(0.041)	(0.035)	(0.069)	(0.068)
FBNC	0.329***	0.330***	0.331***	0.335***	0.320***	0.309***
(1 = yes; 0 = no)	(0.022)	(0.025)	(0.024)	(0.026)	(0.021)	(0.022)
After IIRIRA	1.493***	1.454**	1.370***	1.293*	1.038	1.006
(1 = yes; 0 = no)	(0.144)	(0.176)	(0.111)	(0.141)	(0.234)	(0.239)
Age (years)		1.143***		1.143***	1.142***	1.143***
		(0.017)		(0.016)	(0.015)	(0.015)
Male		0.728*		0.725*	0.729*	0.729*
(1 = yes; 0 = no)		(0.113)		(0.114)	(0.098)	(0.098)
Married		0.652***		0.646***	0.648***	0.659***
(1 = yes; 0 = no)		(0.074)		(0.072)	(0.070)	(0.073)
Unemployment Rate (%)			0.945	0.925+	0.997	0.926
			(0.047)	(0.039)	(0.069)	(0.166)
30-45 Whites w/ some college			2.027*	2.028*	2.114**	1.842**
(1 = yes; 0 = no)			(0.618)	(0.639)	(0.509)	(0.373)
Mexican FBNC in a state (%)			0.990	0.994	1.036	1.045+
			(0.013)	(0.011)	(0.029)	(0.028)
Constant	0.125***	0.006***	0.131***	0.007***	0.005***	0.007**
	(0.008)	(0.002)	(0.023)	(0.002)	(0.003)	(0.010)
Observations	34557	34557	34557	34557	34434	33391

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses.

Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 22-30 of Mexican ancestry. Values are presented in odds-ratios.

Table 5. Main Effects, Bachelor's-Degree Attainment.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ State, Year, Month FE	+ State×Year FE
FBNC after IIRIRA (1 = yes; 0 = no)	0.354*** (0.056)	0.353*** (0.055)	0.315*** (0.046)	0.305*** (0.040)	0.234*** (0.054)	0.213*** (0.051)
FBNC (1 = yes; 0 = no)	0.368*** (0.031)	0.370*** (0.037)	0.373*** (0.035)	0.377*** (0.040)	0.348*** (0.036)	0.329*** (0.032)
After IIRIRA (1 = yes; 0 = no)	1.431** (0.149)	1.384* (0.178)	1.289** (0.105)	1.203* (0.106)	1.015 (0.162)	0.894 (0.146)
Age (years)		1.168*** (0.042)		1.168*** (0.041)	1.167*** (0.038)	1.168*** (0.037)
Male (1 = yes; 0 = no)		0.692** (0.077)		0.684** (0.075)	0.687*** (0.066)	0.705*** (0.065)
Married (1 = yes; 0 = no)		0.630* (0.140)		0.625* (0.139)	0.626* (0.134)	0.631* (0.138)
Unemployment Rate (%)			0.936 (0.075)	0.913 (0.064)	0.942 (0.086)	0.926 (0.216)
30-45 Whites w/ Bachelor's + (1 = yes; 0 = no)			1.199 (0.689)	1.308 (0.693)	1.490 (0.610)	1.332 (0.657)
Mexican FBNC in a state (%)			0.981 (0.022)	0.985 (0.019)	1.060* (0.028)	1.064** (0.019)
Constant	0.067*** (0.006)	0.002*** (0.001)	0.112*** (0.030)	0.003*** (0.003)	0.003*** (0.002)	0.002** (0.005)
Observations	34557	34557	34557	34557	34434	32602

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses.

Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 22-30 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 1. In-State-Resident Tuition (ISRT) Laws.

	ISRT Policy				SFAB Policy		Affirmative-Action Ban
State	Bill Number	Effective Date	Repeal Date	Residence Years	Bill Number	Effective Date	Effective Date
Texas	HB 1403[1]	June 16, 2001	--	3	HB 1403[2]	June 16, 2001	March 18, 1996[3]
California	AB 540[4]	January 1, 2002	--	3	AB 131[5]	January 1, 2013	November 6, 1996[6]
Utah	HB 144[7]	July 1, 2002	--	3	--	--	--
Oklahoma	SB 596[8]	May 12, 2003	November 1, 2007[8 +]	2	--	--	November 7, 2012[9]
Illinois	HB 60[10]	May 20, 2003	--	3	--	--	--
Washington	HB 1079[11]	July 1, 2003	--	1-3	SB 6523[12]	June 12, 2014	December 3, 1998[13]
New York	SB 7784[14]	August 1, 2003	--	2	--	--	--
Kansas	HB 2145[15]	July 1, 2004	--	3	--	--	--
New Mexico	SB 582[16]	April 5, 2005	--	1	SB 582[17]	April 5, 2005	--
Nebraska	LB 239[18]	April 13, 2006	--	3	--	--	November 5, 2008[19]
Wisconsin	AB 75[20]	October 27, 2009	July 1, 2011[20]	3	--	--	--

Connecticut	HB 6390[21]	July 1, 2011	--	4	--	--	--
Rhode Island	* [22]	September 26, 2012	--	3	--	--	--
Maryland	SB 167[23]	November 6, 2012	--	3	--	--	--
Minnesota	SF 723[24]	May 21, 2013	--	3	SF 723[25]	May 21, 2013	--
Oregon	HB 2787-A[26]	July 1, 2013	--	3	--	--	--
New Jersey	SB 2479[27]	December 20, 2013	--	3	--	--	--

Sources: Some information from Farley, Gaertner, & Moses (2013); Flores (2010); Olivas (2004).

Notes: **1.** TEX. EDUC. CODE ANN. § 54.052 (Vernon 2001); **2.** TEX. EDUC. CODE ANN. § 52.32 (Vernon 2001); **3.** *Hopwood v. Texas*, 78 F.3d 952 (5th Cir. 1996); **4.** CAL. EDUC. CODE §§ 68130.5, 69508.5 (West 2002); **5.** CAL. EDUC. CODE § 69508.5(b), (c) (West 2012); **6.** CAL. CONST. ART. I, § 31; **7.** UTAH CODE ANN. § 53B-8-106 (2002); **8.** OKLA. STAT. ANN. TIT. 70, § 3242 (2003; amended 2007); **9.** OKLA. CONST. ART. II, § 36; **10.** 110 ILL. COMP. STAT. ANN. 305/7e-5 (2013); **11.** WASH. REV. CODE ANN. § 28B.15.012 (2003); **12.** WASH. REV. CODE ANN. § 28B.92.010 (2014); **13.** WASH. REV. CODE ANN. § 49.60.400 (1998); **14.** N.Y. EDUC. LAW §355(2)(h)(8) (2002); **15.** KAN. STAT. ANN. §76-731a (2004); **16.** N.M. STAT. ANN. §21-1-4.6(A) (2005); **17.** N.M. STAT. ANN. §21-1-4.6(B) (2005); **18.** NEB. REV. STAT. ANN. § 85-502 (2006); **19.** NEB. CONST. ART. I, § 26; **20.** WIS. STAT. § 36.27 (2007; amended 2011); **21.** CONN. GEN. STAT. §10a-29 (2011); **22.** Rhode Island Board of Governors for Higher Education (2011); **23.** MD. CODE ANN. EDUC. § 15-106.8 (2008); **24.** MINN. STAT. ANN. § 135A.043 (2013); **25.** MINN. STAT. ANN. § 135A.044 (2013); **26.** OR. REV. STAT. § 351.641 (2013); **27.** N.J. STAT. ANN. § 18A:62-4.4 (West 2013);

+ The Oklahoma State Regents for Higher Education has awarded ISRT benefits at its discretion since 2007 (Oklahoma State Regents for Higher Education, 2010); * In 2011, the Rhode Island Board of Governors for Higher Education voted to extend ISRT to undocumented immigrant students.

Appendix 2. Sensitivity, Falsification, and Heterogeneity Analyses, High-School-Diploma Attainment.

	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Traditional States	Top 10 States	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	IIRIRA Signed	PRWORA Enacted	Men	Women	Married	Unmarried
FBNC after IIRIRA	0.373***	0.339**	0.411**	0.840	0.995	0.262**	0.215***	0.323***	0.460***	0.255*	0.379***
(1 = yes; 0 = no)	(0.053)	(0.056)	(0.024)	(0.169)	(0.168)	(0.103)	(0.084)	(0.054)	(0.093)	(0.132)	(0.050)
FBNC	0.269***	0.261**	0.245**	0.644***	1.560**	0.243***	0.249***	0.241***	0.268***	0.227***	0.275***
(1 = yes; 0 = no)	(0.042)	(0.040)	(0.037)	(0.077)	(0.253)	(0.029)	(0.032)	(0.011)	(0.062)	(0.022)	(0.042)
After IIRIRA	1.100	1.094	1.198*	0.793	0.818	0.792	0.637	0.965	1.423**	0.823	1.121
(1 = yes; 0 = no)	(0.128)	(0.149)	(0.072)	(0.138)	(0.140)	(0.264)	(0.232)	(0.141)	(0.177)	(0.333)	(0.136)
Age (years)	1.580***	1.578**	1.570**	2.333***	2.333***	1.589***	1.587***	1.665***	1.537***	1.457***	1.615***
	(0.024)	(0.026)	(0.020)	(0.106)	(0.104)	(0.027)	(0.027)	(0.103)	(0.100)	(0.050)	(0.032)
Male	0.680**	0.678**	0.686**	0.711***	0.709***	0.699***	0.699***			1.250***	0.625***
(1 = yes; 0 = no)	(0.056)	(0.058)	(0.062)	(0.048)	(0.047)	(0.056)	(0.057)			(0.073)	(0.048)
Married	0.369**	0.347**	0.355**	0.577***	0.576***	0.373***	0.373***	0.550**	0.309***		
(1 = yes; 0 = no)	(0.101)	(0.095)	(0.099)	(0.046)	(0.048)	(0.091)	(0.090)	(0.110)	(0.055)		
Unemployment Rate	0.902	0.925	0.850	0.948	0.956	0.972	0.904	1.215	0.717	0.796	1.039
(%)	(0.204)	(0.233)	(0.206)	(0.110)	(0.113)	(0.188)	(0.248)	(0.188)	(0.233)	(0.305)	(0.189)
Whites 30-45, some college (1 = yes; 0 = no)	1.414 (0.524)	1.418 (0.479)	1.256 (0.397)	3.333*** (0.539)	3.671*** (0.590)	1.246 (0.414)	1.209 (0.388)	1.151 (0.421)	1.277 (0.423)	1.761 (0.752)	0.919 (0.240)

Mexican FBNC in a state (%)	0.975 (0.041)	1.025 (0.021)	1.027 (0.025)	1.049^{+} (0.029)	1.049^{+} (0.029)	0.969 (0.035)	0.966 (0.036)	0.960 (0.035)	0.954 (0.036)	0.999 (0.091)	0.957 (0.036)
Constant	0.001** (0.001)	0.000** (0.001)	0.001** (0.001)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.001*** (0.001)	0.000*** (0.000)	0.001* (0.002)	0.000*** (0.001)	0.000*** (0.000)
Observations	11795	10611	10363	159116	153352	13690	13690	6811	6514	2159	11142

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 3. Sensitivity, Falsification, and Heterogeneity Analyses, College Enrollment.

	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Traditional States	Top 10 States	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	IIRIRA Signed	PRWORA Enacted	Men	Women	Married	Unmarried
FBNC after IIRIRA (1 = yes; 0 = no)	0.986	0.973	1.116	0.942	0.860	1.463	0.642	0.754^{+}	1.336	0.881	1.012
	(0.265)	(0.277)	(0.266)	(0.239)	(0.161)	(0.423)	(0.223)	(0.126)	(0.605)	(0.779)	(0.252)
FBNC (1 = yes; 0 = no)	1.103	1.052	1.078	2.352***	1.270	1.056	1.021	1.194	0.997	0.685	1.124
	(0.202)	(0.218)	(0.217)	(0.301)	(0.234)	(0.255)	(0.177)	(0.177)	(0.208)	(0.197)	(0.211)
After IIRIRA (1 = yes; 0 = no)	1.053	0.941	1.066	0.783	0.732^{+}	1.472	0.638*	0.880	1.353	2.003	1.050
	(0.192)	(0.217)	(0.196)	(0.124)	(0.120)	(0.356)	(0.128)	(0.138)	(0.359)	(1.931)	(0.189)
Age (years)	0.388***	0.378***	0.376***	0.264***	0.257***	0.377***	0.378***	0.394***	0.365***	0.532***	0.357***
	(0.030)	(0.031)	(0.033)	(0.015)	(0.014)	(0.029)	(0.029)	(0.023)	(0.038)	(0.050)	(0.030)
Male (1 = yes; 0 = no)	0.962	0.962	0.992	1.380***	1.408***	0.966	0.967			0.993	0.969
	(0.138)	(0.151)	(0.159)	(0.088)	(0.083)	(0.127)	(0.128)			(0.234)	(0.127)
Married (1 = yes; 0 = no)	0.352***	0.354***	0.362***	0.634	0.625	0.353***	0.356***	0.355***	0.348***		
	(0.019)	(0.018)	(0.015)	(0.177)	(0.192)	(0.023)	(0.023)	(0.052)	(0.026)		
Unemployment Rate	0.858	0.843	0.795	1.107	1.080	0.847	0.763	1.181	0.513^{+}	1.158	0.839

(%)	(0.225)	(0.208)	(0.210)	(0.215)	(0.210)	(0.187)	(0.149)	(0.435)	(0.169)	(0.508)	(0.221)
Whites 30-45, some college (1 = yes; 0 = no)	1.229	0.944	0.994	0.549*	0.482**	1.285	1.262	1.876+	0.795	0.319	1.445
	(0.351)	(0.197)	(0.157)	(0.123)	(0.107)	(0.358)	(0.342)	(0.665)	(0.468)	(0.261)	(0.458)
Mexican FBNC in a state (%)	0.997	1.017	1.010	0.935*	0.940+	1.002	0.995	1.023	0.951*	1.000	0.994
	(0.017)	(0.013)	(0.011)	(0.029)	(0.029)	(0.016)	(0.017)	(0.032)	(0.022)	(0.083)	(0.015)
Constant	5.36×10^{7}**	9.63×10^{7}***	1.60×10^{8}***	2.58×10^{10}***	5.43×10^{10}***	9.74×10^{7}***	1.98×10^{8}**	3.62×10^{6}***	7.24×10^{9}***	8.85×10^{3}***	2.82×10^{8}**
	(7.00×10^{7})	(7.83×10^{7})	(1.48×10^{8})	(3.77×10^{10})	(8.35×10^{10})	(1.25×10^{8})	(2.33×10^{8})	(5.31×10^{6})	(2.13×10^{10})	(2.68×10^{4})	(4.08×10^{8})
Observations	20846	19126	18370	275360	264008	23524	23524	12028	10344	4733	17164

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 18-24 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 4. Sensitivity, Falsification, and Heterogeneity Analyses, Associate's-Degree Attainment.

	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Traditional States	Top 10 States	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	IIRIRA Signed	PRWORA Enacted	Men	Women	Married	Unmarried
FBNC after IIRIRA (1 = yes; 0 = no)	0.236**	0.260**	0.301**	1.186	1.764**	0.183***	0.212***	0.217**	0.190***	0.195***	0.229**
	(0.077)	(0.087)	(0.087)	(0.258)	(0.292)	(0.026)	(0.036)	(0.116)	(0.026)	(0.036)	(0.114)
FBNC	0.320***	0.298**	0.306**	1.247+	1.703***	0.336***	0.332***	0.289***	0.320***	0.251***	0.346***
(1 = yes; 0 = no)	(0.020)	(0.023)	(0.017)	(0.151)	(0.196)	(0.023)	(0.020)	(0.025)	(0.023)	(0.021)	(0.040)
After IIRIRA (1 = yes; 0 = no)	1.037	1.159	1.192	0.975	0.975	0.777+	0.938	1.348	0.666***	0.879	1.081
	(0.269)	(0.315)	(0.309)	(0.078)	(0.073)	(0.110)	(0.155)	(0.547)	(0.072)	(0.124)	(0.432)
Age (years)	1.139***	1.137**	1.143**	1.090***	1.085***	1.143***	1.143***	1.172***	1.104***	1.130***	1.156***
	(0.016)	(0.018)	(0.016)	(0.008)	(0.007)	(0.015)	(0.015)	(0.018)	(0.015)	(0.022)	(0.012)
Male (1 = yes; 0 = no)	0.730+	0.710+	0.713+	0.782***	0.763***	0.728*	0.729*			0.709	0.729***
	(0.110)	(0.112)	(0.118)	(0.033)	(0.034)	(0.102)	(0.102)			(0.158)	(0.047)
Married (1 = yes; 0 = no)	0.649**	0.627**	0.632**	1.004	1.000	0.660***	0.660***	0.640**	0.687***		
	(0.080)	(0.078)	(0.081)	(0.035)	(0.037)	(0.075)	(0.072)	(0.104)	(0.037)		
Unemployment Rate (%)	0.994	0.923	1.039	0.835**	0.846*	0.909	0.896	0.942	0.905	1.090	0.747
	(0.192)	(0.207)	(0.168)	(0.055)	(0.058)	(0.172)	(0.177)	(0.341)	(0.188)	(0.414)	(0.233)
Whites 30-45, some college (1 = yes; 0 = no)	1.806*	2.101**	1.828**	11.543***	12.773***	1.892**	1.826**	1.549	1.981**	1.821+	1.673
	(0.342)	(0.406)	(0.251)	(1.135)	(1.230)	(0.350)	(0.398)	(0.528)	(0.463)	(0.577)	(0.542)

Mexican FBNC in a state (%)	1.063^{+}	1.098**	1.106**	1.045*	1.046*	1.044^{+}	1.044^{+}	1.067	1.019	1.052	1.044^{+}
	(0.031)	(0.021)	(0.016)	(0.020)	(0.020)	(0.026)	(0.026)	(0.069)	(0.027)	(0.045)	(0.024)
Constant	0.004**	0.006*	0.002**	0.035***	0.034***	0.008***	0.009**	0.002*	0.025*	0.002*	0.022
	(0.007)	(0.012)	(0.003)	(0.017)	(0.016)	(0.012)	(0.014)	(0.005)	(0.040)	(0.006)	(0.052)
Observations	28613	26912	25644	379268	358801	33391	33391	17321	14672	17509	14687

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 22-30 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 5. Sensitivity, Falsification, and Heterogeneity Analyses, Bachelor's-Degree Attainment.

	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Traditional States	Top 10 States	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	IIRIRA Signed	PRWORA Enacted	Men	Women	Married	Unmarried
FBNC after IIRIRA	0.221***	0.258**	0.305**	1.629*	2.134***	0.222***	0.208***	0.175***	0.206***	0.198***	0.198***
(1 = yes; 0 = no)	(0.056)	(0.053)	(0.039)	(0.333)	(0.319)	(0.061)	(0.062)	(0.070)	(0.040)	(0.041)	(0.057)
FBNC	0.339***	0.323**	0.348**	1.672***	2.080***	0.354***	0.350***	0.300***	0.347***	0.299***	0.337***
(1 = yes; 0 = no)	(0.029)	(0.038)	(0.025)	(0.177)	(0.241)	(0.039)	(0.036)	(0.027)	(0.050)	(0.043)	(0.042)
After IIRIRA (1 = yes; 0 = no)	0.890 (0.160)	1.005 (0.159)	1.103 (0.138)	1.040 (0.067)	1.045 (0.058)	0.878 (0.210)	0.858 (0.214)	1.064 (0.264)	0.658** (0.090)	0.712* (0.109)	1.001 (0.190)
Age (years)	1.166** (0.042)	1.163** (0.045)	1.155** (0.045)	1.100*** (0.007)	1.094*** (0.007)	1.168*** (0.037)	1.168*** (0.037)	1.181** (0.056)	1.151*** (0.021)	1.158*** (0.027)	1.187*** (0.032)
Male (1 = yes; 0 = no)	0.685** (0.069)	0.670** (0.068)	0.668** (0.070)	0.811*** (0.022)	0.791*** (0.022)	0.706*** (0.067)	0.706*** (0.067)			0.721+ (0.140)	0.675*** (0.035)
Married (1 = yes; 0 = no)	0.637 (0.161)	0.628 (0.170)	0.631 (0.176)	0.922* (0.032)	0.915* (0.037)	0.633* (0.140)	0.633* (0.138)	0.644 (0.205)	0.610*** (0.048)		
Unemployment Rate (%)	1.041 (0.277)	0.925 (0.269)	0.952 (0.273)	0.910* (0.041)	0.917* (0.038)	0.897 (0.206)	0.869 (0.184)	0.793 (0.262)	1.135 (0.275)	1.181 (0.497)	0.732+ (0.135)
Whites 30-45, some college (1 = yes; 0 = no)	1.809 (0.871)	1.777 (0.821)	1.986 (0.805)	127.783*** (18.325)	147.754*** (20.136)	1.317 (0.635)	1.325 (0.641)	2.682 (2.038)	0.520 (0.217)	3.524 (4.571)	0.713 (0.408)

Mexican FBNC in a state (%)	1.085*** (0.017)	1.093** (0.024)	1.109** (0.024)	1.025^{+} (0.015)	1.024^{+} (0.013)	1.064*** (0.018)	1.063** (0.020)	1.107 (0.068)	1.013 (0.052)	1.092* (0.042)	1.019 (0.030)
Constant	0.001* (0.002)	0.002* (0.005)	0.002* (0.004)	0.017*** (0.006)	0.018*** (0.006)	0.003** (0.006)	0.004** (0.006)	0.002* (0.005)	0.002*** (0.003)	0.000** (0.001)	0.009** (0.013)
Observations	28403	26616	25407	379268	358801	32602	32602	16565	14140	16692	14073

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1994-2001, and exclude observations taken after Texas enacted the first in-state-resident-tuition (ISRT) law in June, 2001. Except as otherwise noted, all observations are of respondents ages 22-30 of Mexican ancestry. Values are presented in odds-ratios.

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DREAMbuilders?: The effect of in-state-resident tuition policies on undocumented-students' educational attainment

Matthew Patrick Shaw

2016

I. Introduction

In 1996, Congress passed the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA). IIRIRA states that undocumented immigrants[16]:

> "shall not be eligible on the basis of residence within a State…for any postsecondary education benefit unless a citizen or national of the United States is eligible for such benefit…without regard to whether the citizen or national is such a [state] resident"

(codified at 8 U.S.C. § 1623(a)). Because of IIRIRA, most aspiring undocumented college students have to pay higher out-of-state tuition costs that equate to nearly half their family's annual income. In an earlier paper, I found that IIRIRA depressed high-school graduation odds of likely undocumented students of Mexican ancestry two-and-a-half fold and associate's-degree and bachelor's-degree odds four-and-a-half fold each, all things equal (Shaw 2016a). To make matters worse, undocumented students cannot receive federal financial aid to help meet these higher costs because of the Personal Responsibility and Work Opportunity Act of 1996 (PRWORA, codified at 8 U.S.C. § 1611). As a result, most undocumented students have to pay for college costs out-of-pocket, or rely upon state-based financial aid programs and a growing, but still insufficient supply of private scholarships (Hernandez et al. 2010; Perez 2010). Unsurprisingly, fewer than 10 percent of undocumented students are able to secure

[16] An undocumented immigrant is a non-U.S.-citizen or national who is "unlawfully present" in the U.S. Undocumented immigrants include persons who migrated to the U.S. without obtaining a "green card" or an immigrant visa and persons who obtained non-immigrant visas and overstayed the authorized time period. Undocumented minor children, though technically incapable of being "unlawfully present" in the U.S. (see 8 U.S.C. § 1182(a)(9)(B)(iii)(I)), cannot obtain authorizing papers (e.g., social security number) necessary to work or obtain certain licenses (8 U.S.C. § 1182).

sufficient funding to cover the full cost of attending college (Dougherty, Nienhusser and Vega 2010).

Some states have intervened to relieve undocumented students of IIRIRA-imposed higher out-of-state tuition costs by passing in-state-resident tuition (ISRT) laws. While most states do not offer ISRT benefits to undocumented students, as of 2013, 17 states do. To satisfy IIRIRA, states must offer ISRT to *all* non-state residents who meet certain criteria. Because these states offer ISRT benefits to non-state resident U.S. citizens and legal residents, they can offer these benefits to undocumented in-state residents.

In this paper, I examine the impact of ISRT on four educational attainment outcomes for undocumented young adults: high-school graduation (for 18-21 year-olds), college enrollment (for 18-24 year-olds), associate's-degree attainment and bachelor's-degree attainment (both for 22-30 year-olds).

I do so using data from the Current Population Survey Merged Outgoing Rotating Groups (CPS-MORG) from July 1998, when IIRIRA went into effect until December 2013. I use a generalized difference-in-differences (DD) strategy that takes the differences in enrollment and degree attainment for foreign-born non-citizen (FBNC) youth of Mexican ancestry, both before and after ISRT policies by state, takes the same differences for the same outcomes for U.S. citizen and legal residents of Mexican ancestry, and then takes the difference of the two differences to estimate the impact of policies that lower college tuition cost on educational attainment among the undocumented. Because federal financial aid remains unavailable to undocumented residents due to PRWORA, this study offers a unique exploration into the role of tuition and tuition sensitivity in affecting

college enrollment in the absence of aid. Because CPS-MORG observations of FBNC youth of Mexican ancestry is the best and most commonly used proxy for the likely undocumented in available national data sets, these differences-in-differences provide the strongest estimate of ISRT policies' effect on each outcome.

This study also complements existing legal and empirical research on ISRT policies in a number of ways. First, by using the generalized DD estimation strategy, I preserve between-state variation in ISRT and other policies in measuring policy effects as opposed to treating all states and their ISRT policies as the same. Second, this study is also one of the first to explore ISRT effects on bachelor's degree attainment, an important mechanism for socioeconomic mobility. Third, and related to this, this study is the first to look at multiple education outcomes across the transition into early adulthood. Because this transition occurs simultaneously with a transition by many undocumented youth into conscious "illegality," studying multiple outcomes along this transition enables me to discuss how law operates to constrain or facilitate opportunity for these young people. Fourth, because more states have enacted ISRT policies since the first wave of research, this paper is able to more fully explore state-level variation in policies to better understand how ISRT influences undocumented-student enrollment and graduation.

I find that ISRT induced higher rates of high-school graduation among Mexican FBNC respondents ages 18-21, as compared to citizens of Mexican ancestry of the same age, but they did not lead to meaningful improvement in college enrollment rates among Mexican FBNC ages 18-24 or associate's-degree and bachelor's-degree attainment among Mexican FBNC ages 22-30, when compared to age-cohort citizens of Mexican ancestry. These results suggest that while tuition subsidies might encourage students to

consider college, without more they are insufficient to yield higher enrollment or graduation rates. My findings have implications for state and federal laws impacting college-aged undocumented youth, how we understand tuition to influence college decisions among vulnerable communities, and laws on education rights. I discuss some of these in the conclusion.

This paper proceeds as follows. Section II summarizes the background of laws affecting education of the undocumented in the U.S., provides context on ISRT policies, including existing research, and details how this study aids in our understanding of ISRT policies. Section III describes the theoretical framework motivating this study. Section IV describes the research design, including data, analytical strategy, and threats to validity. Section V discusses results, and Section VI concludes with implications.

II. Background and Literature Review

<INSERT FIGURE 1 HERE>

Laws Framing the Access of Undocumented Students to Education in the U.S.

Undocumented immigrant youth in the U.S. are guaranteed a free, publicly funded K-12 education (*Plyler v. Doe* 1982). In *Plyler*, the U.S. Supreme Court speaks sweepingly of the "pivotal role of education" and its fear that without a basic, high-school education, undocumented persons are more likely to be unemployed, dependent on governmental assistance, and commit crime. Against these harms, the Court rejected Texas's claims that educating undocumented youth imposes special burdens on state resources or reduces the quality of education states may offer to U.S. citizens and legal residents. Finding that states cannot use law to create a "discrete subclass of illiterate

persons," a five-justice majority invalidated a Texas law that defunded undocumented students' elementary and secondary education and a local district policy that required them to pay tuition costs their citizen and legal-resident peers did not (p. 230). The Family Education Rights and Privacy Act of 1974 (FERPA) offers additional protections for undocumented children. Schools have an affirmative duty to shield information about students' and their families' residency status from disclosure—often from the students themselves (codified at 20 U.S.C. §1232).

Because of these laws, 65,000 of the estimated 80,000 undocumented high-school-age youth in the U.S. receive a high-school diploma each year (Passel 2005; and Cohn 2009). As undocumented youth approach graduation and adulthood, these protections and guarantees expire, and are replaced with much harsher governmental regulations on their employment, ability to receive governmental assistance, and in some states, their ability to obtain a driver's license (Gonzales 2009). For undocumented students, many of whom have grown up not knowing their residency status, discovering their status and its implications on their adult lives raises new uncertainties about their future (see Altonji 1993, in Flores 2010; Gonzales 2009). Emergent liminal legality, magnified by IIRIRA restrictions on financial aid and in-state-resident-tuition eligibility, combine with these uncertainties to constrain college decision among undocumented young adults. These phenomena are believed to explain why fewer than 20 percent of undocumented youth ever attempt post-secondary education (Passel 2003).

Cost, which is a primary barrier to obtaining a college degree for many students, is another important factor to consider (Baum, Ma and Payea 2013; Deming and Dynarski 2010). Evidence suggests that undocumented students are even more sensitive

to tuition and financial aid changes than their citizen and legal-resident peers (Gonzales 2009; 2011; Teranishi et al. 2015). For the average undocumented student, whose family earns 30 percent less than the average American family, in-state-resident tuition is 18 percent of the $35,000 annual family income. With room and board, in-state-resident tuition costs amount to 39 percent of family income (Passel and Taylor 2010). Recall, that because of IIRIRA, most undocumented students in the U.S. are ineligible for in-state-resident tuition. Therefore, aspiring undocumented students must typically pay out-of-state costs. Out-of-state tuition and fees alone are 48 percent of an undocumented family's annual income. With room and board, the out-of-state cost is practically unaffordable, amounting to 69 percent of family income. Because of PRWORA (8 U.S.C. § 1611), undocumented students cannot receive federal financial aid. Instead, most undocumented students pay for college costs out-of-pocket, or rely upon a small number of state-based financial aid programs and private scholarships (Hernandez et al. 2010; Perez 2010). As stated in the introduction, fewer than 10 percent of undocumented students are able to cover the full cost of attending college (Dougherty et al. 2010)

Federal law currently offers no guidance on whether states must allow undocumented students to enroll in public colleges. In the absence of clear direction, the states have responded in a variety of ways. While most are officially silent, some prohibit undocumented students from enrolling in any public college (e.g., S.C. CODE ANN. §59-101-430 (2008) (South Carolina)), others limit their enrollment to non-selective colleges (e.g., University System of Georgia 2010), and others to community colleges only (e.g., 23 N.C. ADMIN. CODE. 02C.0301 (2010) (North Carolina)). Individual undocumented students attempting to enroll in colleges across the country report idiosyncratic barriers in

the forms of administrators requesting their social security numbers, or, alternately, help from different personnel to meeting or circumventing bureaucratic hurdles. These policy and practice uncertainties couple with unique barriers presented by IIRIRA to influence undocumented youth's decisions to enroll and persist in college differently from their peers (Abrego and Gonzales 2010).

This is not to say that there has been no activity at the federal level to address this situation. In 2003, U.S. Senators Dick Durbin of Illinois and Orrin Hatch of Utah introduced a bill that, if enacted, would become the Development, Relief, and Education for Alien Minors (DREAM) Act. As proposed, the DREAM Act would amend IIRIRA to allow states to determine for themselves, without condition, whether undocumented residents qualify for in-state-resident tuition. The Act would also amend provisions from PRWORA, and allow undocumented students access to federal loans and work study, but not grants. Additional provisions address authorizing employment and outline pathways to legal residency and citizenship (S. 1297, 107th Cong. 2001). Multiple iterations of the DREAM Act have been introduced in both houses of Congress, and have failed, at times by narrow margins. The DREAM Act's most recent failure, in June 2011, led the Obama administration to institute the Deferred Action on Childhood Arrivals (DACA) program. DACA does not affect laws on tuition cost and financial aid. It instead allows eligible undocumented youth to stay in the U.S. for two years to attend school and work, and stays deportation proceedings.

<INSERT FIGURE 2 HERE>

The Development of ISRT Laws

Seventeen states have also responded to IIRIRA by passing ISRT bills. On their face, each allows a subcategory of non-state residents to qualify for in-state-resident tuition without first establishing state residency. Most ISRT laws contain five eligibility provisions: a student (1) resided in the state *at some point* for a number of years; (2) during some of which he attended high school in the state; (3) holds a high-school diploma or a GED; (4) seeks to enroll in a public college or university; and (5) indicates an intent to pursue in-state residency status as soon as he is eligible.

These provisions satisfy IIRIRA by providing an alternate path to in-state residence for citizen and legal residents to access ISRT. Note that IIRIRA requires the same procedural path to benefits, and does not require whatever path a state chooses to actually confer the same likelihood of access. As I show in Appendix 1, most states require three years of previous in-state residency to qualify for ISRT. Because a student who graduates from an in-state high school is typically eligible for ISRT as an in-state resident, most residency year requirements exclude all but a small subset of former residents who moved out of state after three years of high school. New Mexico is the most generous ISRT state, requiring only one year of in-state residence while in high school (N.M. STAT. ANN. §21-1-4.6(A) (2005)). Connecticut is the strictest, requiring four years—the typical duration of high-school (CONN. GEN. STAT. §10a-29 (2011)). By doing so, Connecticut effectively excludes former in-state-resident U.S. citizens and legal residents from accessing ISRT. Undocumented students who have spent most of their childhood in the U.S. are typically able to meet state residency requirements. Youth who

arrive after the first year of high school (or after middle school in Connecticut) are usually ineligible for ISRT immediately upon graduation.

Immigrants, including the undocumented, are less likely to migrate between states once they arrive to the U.S. (Kritz and Nogle 1994), even after the passage of ISRT laws (Nair-Reichert and Cebula 2015). Much of recent development in undocumented migration to states that have not historically seen concentrated immigration is fueled by direct migration from foreign countries and not domestic migration. In their recent paper, Nair-Reichert and Cebula (2015) find that while undocumented immigrants are more likely to migrate within the U.S. to states with greater economic opportunities or with lower tax burdens so as to maximize their spending power, the relationship between a state having an ISRT policy and across-state migration is negative, all things equal. Though many ISRT states also have stronger undocumented community and advocacy networks, favorable tuition policies do not induce migration. The authors explain that within ISRT-policy states there might be greater competition over fewer educational resources, which discourages across-state migration. Because of these features and migration patterns among the undocumented, undocumented students of Latin American origin, in particular Mexican origin, have been ISRT laws' primary beneficiaries (Flores and Chapa 2009).

While states with conservative Republican majorities and lower proportions of Latino voters are known to be more likely to pass restrictive legislation in the guise of immigration enforcement, patterns characterizing states likely to pass ISRT laws have been difficult to find. Importantly, evidence does not suggest that the passage of ISRT laws is endogenously driven by in-state undocumented-community activism or

population proportions of the undocumented community lending credence to the enactment of ISRT laws being exogenous to the target undocumented college-aged population (McLendon, Mokher and Flores 2015). Of the eight states with the highest percentage of undocumented immigrants, four were early passers: California (24 percent 2001), Texas (14 percent 2001), New York (7 percent 2003), and Illinois (4 percent 2003); and four hadn't passed ISRT as of 2013: Florida (9 percent, passed in 2014); Arizona (5 percent), New Jersey (4 percent), and North Carolina (3 percent). States with Democratic governments have been as likely to pass ISRT as states with Republican governments. To illustrate, Republican-controlled Texas was the first state to pass ISRT in 2001, by a vote of 142-1 in the House and 30-0 in the Senate (TEX. EDUC. CODE ANN. § 54.052 (Vernon 2001); followed quickly by Democrat-controlled California (CAL EDUC. CODE §§ 68130.5; 69505.5 (2002)) and Republican-controlled Utah (Utah Code Ann. § 53B-8-106 (2002)). New Jersey, where the legislature has been controlled by Democrats since before 2000 and most of the governors during that time were of the same party, did not pass ISRT until December 2013 (N.J. STAT. ANN. §18A-62-4.4 (West 2013) (cf. McLendon, Mokher and Flores 2015). There is no evidence of state-to-state policy diffusion (McLendon et al. 2015), nor is there an apparent regional trend. The most palpable trends among ISRT states are higher numbers of higher concentration of eligible Latino voters, more female legislators, and decentralized university governance (McLendon et al).

Lessons from Research on ISRT Laws and College Enrollment

Existing studies generally show that ISRT reverses high-school dropout likelihoods and increases undocumented-student college enrollment. None to my

knowledge has examined four-year-degree outcomes. Five studies examining Current Population Study Merged Outgoing Rotation Group (CPS-MORG) data find positive enrollment effects: two for high-school outcomes (Bozick and Miller 2014; Potochnick 2014) and three for college outcomes (Amuedo-Dorantes and Sparber 2012; Flores 2010; Kaushal 2008). A sixth study, examining American Community Survey (ACS) data, finds no effect (Chin and Juhn 2011).[17] As Chin and Juhn (2011) acknowledge, their no-effect finding might be due to the relative newness of both the ACS dataset—it was first collected in 2000—and the relatively short pre- and post-policy periods they could measure in their four-year analytic window. As I discuss below, additional differences in results are likely associated with differences in study date ranges and selection of treatment and control groups.

Kaushal (2008) is generally thought to be the first study examining ISRT-policy effects. Using availability to ISRT policy (not eligibility for ISRT) as her independent variable, Kaushal found that ISRT caused a positive difference in enrollment likelihoods for both FBNC and U.S. citizens of Mexican ancestry. An important consideration is that because of how early in the national ISRT development Kaushal performed her study, she could only examine the impact of ISRT in the first eight states, which, unlike later implementers, tend to have long histories of immigration, particularly Mexican, and stronger social and political networks (Abrego and Gonzales 2010).

Examining only enrollment outcomes, Flores (2010) improved upon Kaushal (2008) by restricting the analytic time window by one year to 1998-2005 because of the

[17] Additional studies offer no clarification on the general effect of ISRT laws on college behaviors as they are either institution- or state-specific (e.g., Conger & Chelman 2013 (examining CUNY); Flores 2007; Dickson & Pender 2013 (Texas universities)) or focus on high school outcomes (Bozick & Miller 2014; Potochnick 2014).

IIRIRA effective date, and dropping 17-year-olds from the sample because they are less likely as an age cohort to enroll in college than 18-24 year-olds. She also limited her sample of FBNC youth to those likely to meet their respective state's ISRT eligibility criteria. Using Latino FBNC as her proxy for undocumented, Flores found that undocumented students are 1.54 times as likely to enroll in college following ISRT enactment. Flores also included measures for affirmative-action factors which may impact enrollment. Amuedo-Dorantes and Sparber (2012) examined enrollment outcomes adjusting the analytic time window to 1999-2010, which they justified as incorporating a potential lag in policy uptake following IIRIRA. Using month-fixed-effects, the authors found a 2.0 percentage-point increase in Mexican FBNC enrollment after ISRT—and similar improvement for non-Latino U.S.-born citizens.

Examining post-IIRIRA data on high-school dropout rates, Potochnick (2014) finds that ISRT policies lead to a 20 percent decline in dropouts among Mexican FBNC youth ages 16-19. Bozick and Miller (2014) find that youth living in states with outright bans on in-state-resident tuition are 49 percent less likely than states without any law to be enrolled in high school, and that youth living in states with ISRT laws are 65% more likely than control-state-group youth to be enrolled in high-school.

III. Theoretical Framework, Hypotheses, and Research Questions

In an earlier paper, I offered a blended framework to understand how undocumented students' decisions to pursue educational opportunities were shaped by anti-immigration laws like IIRIRA (Shaw 2016). Building on Abrego (2006; with Gonzales 2010) and Gonzales's (2009; 2016) work to define liminal legality, I propose

that law operates in a manner unique to undocumented students—even more so than their parents—to create "illegality"[18] as a master status. First, the status of being undocumented and attachment of "illegality" to that status is entirely a creation of law. Unlike any other status group, undocumented people's entire existence as such is defined by statute and administered by regulations. This makes undocumented people's entire existence precariously subject to the whims, exactness, and bluntness of laws and its administrators. Second, because of *Plyler*, FERPA, and other policies and practices that seek to suspend the specter of "illegality" on children, many undocumented children grow up largely, if not completely, unaware of their status, even more grow up unaware of its direct consequences on their adult lives (Abrego 2006; Chavez 1998; Menjívar 2006). More importantly, they grow up alongside their citizen peers with the same goals, aspirations, and expectation that the rights their peers enjoy are also available to them (Gonzales 2009; 2016). Third, as I showed in my previous paper, the enactment of IIRIRA and its imposition of a cascade of barriers triggered by adulthood to education and work begin a series of exclusions, denials, and hurdles that make the costs of college prohibitive and the returns practically nonexistent. An early exiting high-school student is often better off financially and psychologically than a student who attempted to persevere to degree attainment. He tends to find himself years later at the same starting place his early-exiting peer once was, but without the work experience or the cultural capital within the clandestine community of work that his cohort now possesses (Gonzales 2016 in Shaw 2016).

[18] "Illegality" refers only to the positivist law notion of a residence status which exists outside the legal constructs of a given nation-state. My use of the term does not imply that "illegality" is a personal trait or a value (Dabach 2015; see also de Genova 2002).

I hypothesize that IIRIRA modifies the college decision equation for undocumented students in two distinct ways: one affecting the cost side of the equation, and the other the benefits side. First, it removes the tuition subsidy that in-state residents would typically receive to reduce their cost of attendance. Because these students are already foreclosed from participating in the federal financial aid system, simply being an undocumented student makes college less affordable. Second, because of provisions federalizing employment eligibility, IIRIRA makes it less likely for the undocumented to gain access to high-paying jobs whether or not they obtain a college degree. ISRT intervenes to restore tuition subsidy access to many, but not all undocumented students who live in policy states. For the rational aspiring student, whose ability to afford college lies within the marginal difference between out-of-state and in-state-resident tuition, restoring the in-state tuition subsidy would weigh in favor of him attending and persisting in college.

In this paper, I explore the limits of a cost intervention for improving undocumented students' educational attainment within an übersystemic environment of "illegality" (Reskin 2012). In doing so, I apply a variation of the limited legality framework as applied to college decision. Because the undocumented experience is entirely a creation of law, law is uniquely plenipotentiary with regards to improving this experience (e.g., *Plyler*), worsening this experience (e.g., IIRIRA), or doing away with the experience entirely (e.g., IRCA). ISRT laws, at least on paper, appear to be ameliorative in nature. In light of what I termed the metastatic nature of IIRIRA to make residency status more salient on life outcomes than factors which would otherwise predict

success and fulfillment (Shaw 2016), I will evaluate ISRT laws as curative of barriers IIRIRA imposes on the college decision process (see generally Becker 1993).

To investigate whether ISRT has an impact on undocumented students' educational attainment, I explore the following questions:

(1) what effects does offering ISRT have on high-school-diploma attainment among likely undocumented students of Mexican descent age 18-21 in states that offer ISRT, ;

(2) what effects does offering ISRT have on college enrollment among likely undocumented students of Mexican descent age 18-24 in states that offer ISRT, ;

(3) what effects does offering ISRT have on associate's-degree attainment or higher among likely undocumented students of Mexican descent age 22-30 in states that offer ISRT, ; and

(4) what effects does offering ISRT have on bachelor's-degree attainment or higher among likely undocumented students of Mexican descent age 22-30 in states that offer ISRT, in each instance compared to similar students in states that do not offer ISRT?

IV. Research Design

A. Data

I use individual-level data from the CPS-MORG for the years 1998-2013 in my analyses, excluding observations taken before IIRIRA's July 1, 1998 effective date (see Bozick and Miller 2014; Potochnick 2014 (for similar); see also Shaw 2016). These data contain information about educational attainment, enrollment, employment, national

origin, sex, marital, and citizenship/residency status from a multistage stratified sample of approximately 60,000 households per month (U.S. Census Bureau 2015). I supplement CPS-MORG data with U.S. Bureau of Labor Statistics (BLS) data on monthly labor market conditions over the same 15-year time period.

The CPS samples by state-level clusters and by primary-sampling-unit strata within states; and weights by a complex formula that takes age, sex, race, and ethnicity into account to approximate national population representativeness (Judson and Swanson 2011; Schmidley and Robinson 2003). Because of these features, many scholars who examine CPS data use the provided weights to correct sampling bias. However, CPS weights do not account for national origin or citizenship, which are the most important variables for estimating demographic change or related effects on the undocumented population (Van Hook, Zhang, Bean and Passel 2006). Further, no governmental agency separately counts the undocumented. As a result, the CPS undercounts the foreign-born and the undocumented even more so. Despite differences between my population of interest and the U.S. population for which weights were derived (see Davern, Jones, Lepowski, Davidson and Blewett 2007), weights might prove useful in generalizing results using these data to the U.S. population.

Though the CPS does not count the undocumented separately, it is still useful because it inadvertently includes undocumented immigrants among its sample of foreign-born non-citizens (FBNC). Because CPS does not ask about residency status, there is no direct inducement for respondents to lie about their citizenship or country of origin. One important consequence of this might be more undocumented people answering the survey than there would be if there was a clearer threat of disclosure (cf. Passel and Cohn 2009).

Further, CPS data also contain information about individual and parental nativity, and citizenship status that can be used to proxy for undocumented status. As a result, this survey is one of the most important governmental data sources for information about my population of interest because of information it contains about individual and parental nativity, and citizenship status (Passel 2005). These features give the CPS-MORG inherent value and advantages in measuring phenomena involving the undocumented community over other governmental data sources such as the ACS.

B. Measures

1. Outcome Measures

As in my IIRIRA study (see Shaw 2016), I have four binary (1 = yes, 0 = no) outcome measures: (1) having a high-school diploma ($HSDIPLOMA$), (2) being enrolled in college ($COLLENROLLED$), (3) having an associate's degree or higher ($ASSOCIATES$), and having a bachelor's degree or higher ($BACHELOR$).

2. Policy Effect Measure

My policy effect measure is a difference-in-difference estimator, the interaction of two binary measures, $FBNC \times ISRT$ (cf. Shaw 2016). The first of these, $FBNC$, measures all foreign-born non-citizens observed in a sample. This is the treatment assignment proxy for being undocumented (1 = yes, 0 = no). After Passel (2005; and Cohn 2009; and Taylor 2010), I adapt the "residual method" to available data in estimating $FBNC$. This is a conventional method for estimating the foreign-born in the absence of data of residents who do not have legal documents (Deardorff and Blumerman 2001; Judson and Swanson 2011; Schmidley and Robinson 2003). Consistent with this method, I subtracts from the total foreign-born population persons ($ForeignBorn$) all

who indicate: (1) citizen status ($Citizen$) by (a) birth in a U.S. territory[19], (b) one or both parents' citizenship or birth in the U.S. or a U.S. territory[20], or (c) citizenship in the U.S., presumably by naturalization; or (2) citizenship or birth in Cuba[21] ($Cuban$). The remainder, or residual per Passel et al.'s parlance, are all foreign-born non-citizens (FBNC), the category that contains undocumented respondents.

(1)

$$FBNC = ForeignBorn - Citizen - Cuban$$

The strongest proxy for undocumented status in the CPS is being FBNC and of Mexican origin (Kaushal 2008; Potochnick 2014). Passel and Cohn (2009) estimate that around 56 percent of FBNC Mexicans are undocumented. Among FBNC Mexicans who have lived in the U.S. for fewer than 10 years, 80 percent are estimated to be undocumented (Passel and Cohn 2009, in Potochnick 2014). Undocumented Mexicans account for approximately 59 percent of the U.S. undocumented, with non-Mexican undocumented accounting for another 22 percent (Passel and Cohn 2009). These shares have remained constant for the last three decades (Passel and Cohn).

Despite these efforts, the $UNDOC$ variable inadvertently includes foreign-born non-citizens lawfully resident in the U.S. on various green cards, visas, refugee, or other

[19] Guam, the Panama Canal Zone (until 1999), Puerto Rico, and the U.S. Virgin Islands (see 8 U.S.C. § 1401).

[20] CPS-MORG data does not provide information about whether an individual's parents were married or if s/he was born in wedlock. Without this information, there is potential measurement error in the estimation of who is a lawful foreign-born resident (see *Nguyen v. INS* (2001), the U.S. Supreme Court upholding portions of the Immigration and Nationality Act of 1952, codified at 8 U.S.C. § 1409, that maintain more restrictive citizenship and residence requirements for children born out of wedlock to a foreign mother and a U.S. father than for out-of-wedlock children born to a foreign father and a U.S. mother). However, the error that would be introduced by not identifying a foreign-born child of a U.S. citizen parent as having lawful right to residence is far greater than that which might have been introduced

[21] The Cuban Adjustment Act of 1966, codified at 8 U.S.C. § 1255, generally allows Cubans to legally emigrate to the U.S. and establish residency. Though the Cuban Migration Agreement of 1996 allows the U.S. to repatriate any Cubans who fail to reach U.S. territory in their attempt to emigrate, successful migrants are typically allowed to adjust and, after one year, are eligible for permanent resident status under the 1966 Act.

temporary status. As a consequence, estimates of undocumented-student effects using these data are downwardly biased. That is, it is more difficult to find a policy effect on undocumented persons than it would be if one could distill the FBNC categories to remove non-citizen lawful residents. This is a common, well-noted limitation that exists, frankly, out of the very status and stigma of being undocumented (see Amuedo-Dorantes and Sparber 2014; Chin and Juhn 2011; Flores 2010; Kaushal 2008; Potochnick 2014 (all evaluating post-IIRIRA state-level in-state-resident-tuition laws)).

Kaushal (2008) provides a formula to estimate a rough effect size of the bias:

(2) $$Bias = Lawful + [(FBNC - Lawful) * UndercountRate]$$

Estimating that approximately 20 percent of Mexican FBNC are in fact lawful residents, and acknowledging Passel's (2005) estimates that the FBNC measure undercounts Mexican undocumented by around 10 percent, any policy effect calculated from a measure using Mexican FBNC as a proxy for the undocumented absorbs an 28 percent downward bias (Kaushal 2008). To adjust for this bias, a truer predicted policy effect would be 1.28 times an estimated value (Kaushal 2008; Potochnick 2014). Despite this coefficient correction, this unavoidable measurement error increases the standard error, and with it the likelihood of Type II error (Wooldridge 2010). As a result, I am more likely to fail to reject the null hypothesis when there is an actual policy effect. The second variable in the interaction, $ISRT$, indicates whether the respondent was surveyed in an ISRT state during a time in which ISRT was effective (1 = yes, 0 = no).

C. Sampling

To account for identifying the undocumented only among the eligible foreign-born population of Mexican origin, I restrict my samples to respondents of Mexican

origin. These are survey respondents who indicate: (1) birth in Mexico; (2) both parents' birth in Mexico; (3) one parent's birth in Mexico and the other's birth outside of the U.S., its territories, and Cuba; or (4) Mexican ancestry. While this methodological adjustment allows me to reduce the effects of any measurement bias associated with ethnicity and country of origin that might otherwise affect my observations, it also permits a more apples-to-apples comparison of youth who are more likely to have shared similar peers and educational experiences, be of similar socioeconomic backgrounds, and have experienced similar responses to structural and cultural aggressions and discriminations, including a presumption of illegality (see de Genova 2004).

To conform my samples to ISRT criteria, I further limit my samples to respondents who have a high-school diploma or its equivalent, and meet state residency requirements. I identify eligible residents among the CPS as: (a) all U.S.-born citizens, (b) all foreign-born citizens who have resided in the U.S. for at least one year, consistent with state residency rules for citizens; and (c) all foreign-born noncitizens who have resided in the U.S. for (i) the requisite number of years for eligibility under the laws of states that have one point had ISRT, and (ii) for three years under the laws of states that have never had ISRT to reflect general convention.

Importantly, I am not able to identify interstate migration, a key factor in determining residency-based eligibility. While the CPS captures information on interstate migration annually in its Annual Social and Economic Supplement, the monthly CPS and CPS-MORG files do not. My decision to code all native-born citizens and all foreign-born citizens who have lived in the U.S. for at least one year as ISRT eligible takes into account recent research, which shows a steady decline between 1991 and 2011 in

interstate migration, from 3 to 1.5 percent (Kaplan and Schulhofer-Wohl 2015). Even among immigrants who have moved to the U.S. in the last three years, interstate migration has declined from around 5 percent to 3 percent over the same time period (Kaplan and Schulhofer-Wohl). As a result, all variation in residence-based eligibility is identified from time in the U.S. among the foreign-born. In light of evidence that few citizen and legal-resident youth avail themselves of ISRT laws to gain access to in-state-resident tuition, I contend that my coding scheme introduces minimal measurement deviance. Further, to the extent it might be consequential, such deviances bias against my finding an ISRT effect.

Given this, for my analyses of high-school diploma attainment, I include all CPS-MORG respondents of Mexican ancestry between the ages of 18 and 21; and similar respondents between the ages of 18 and 24 for college enrollment; and between the ages of 22-30 for degree attainment outcomes.

D. Analytic Strategy

Unlike in my estimation of a single policy effect in my IIRIRA study (see Shaw 2016), in this paper I am evaluating the impact of multiple events that though similar affected different cohort groups within my population of interest at different times. While the standard DD estimator captures the average intended treatment effect across multiple events the same as it would across a single law or policy event, a lower-order binary term would treat all before-policy observations and all after-policy observations the same, ignoring important state and time variation among them.

In their paper analyzing the aggregate impact of state antitakeover laws, Bertrand and Mullainathan (2003) offer a solution to this problem through a generalized form of the DD model that accounts for state fluctuations in policy and between-year variation:

(3)

$$y_{ist} = \beta_0 + \beta_1(FBNC \times ISRT_{ist}) + \beta_2(FBNC_{ist}) + \beta_3(state_s \times year_t) + \beta_4(X) + \beta_5(Z) + state_s + year_t + month_t + e_{ist}$$

In Equation (3), as in the standard DD model, $FBNC \times ISRT_{ist}$ is my main predictor of interest: an individual i being a foreign-born non-citizen living in state s on or after time t when ISRT was enacted, and β_1 is the causal effect estimator of ISRT's effect on an educational attainment outcome.

In a generalized DD model, instead of lower-order terms measuring average differences in the before-policy treatment group and after-policy control group, fixed effects are specified to account for variation in both groups (Bertrand and Mullainathan 2003). Because a fixed-effects specification for the binary $FBNC$ variable is the same as in a standard DD model, β_2 continues to estimate the average before-ISRT difference in log odds for a foreign-born non-citizen from a citizen on a given outcome. The measure for $ISRT$ is somewhat more complicated, as it is itself an interaction of binary measures: whether a state would eventually pass an ISRT law, and if such a law was effective at the time a participant was surveyed. Because of this, modeling state and year fixed effects alone (or in combination) is insufficient to identify log-odds differences in state policy by year needed to control for non-policy-related effects. To properly identify the general control effect while allowing for state variation, I interact state and year dummies, $state \times year$, with β_3 serving as a placeholder for these state-by-year trends. β_4

measures adjusted differences in log-odds along the covariate matrix X, which measures time-varying individual characteristics such as age, gender, marital status. β_5 measures adjusted differences in log-odds along the covariate matrix Z, which measures time-varying state characteristics such as general and community education attainment trends, migration trends, and unemployment rate. I include time-invariant state fixed effects and state-invariant year effects both as lower-order terms for the state-by-year interaction and to account for fixed between-state and between-year variation, respectively. I include month fixed effects to account for variation in educational attainment across an academic year, and e is an error term.

This approach has three important advantages over a fixed-effects DD model. First, it relaxes both the pre-policy parallel trends assumption and the post-policy assumption that these different state ISRT laws enacted on a staggered timeline are likely to have the same effect across the years. Second, it accommodates differences between FBNC and U.S. citizens of Mexican ancestry, otherwise unobserved variation between the states, as well as common differences by year, allowing for a more precise policy-effect estimate. Third, allowing for this variation also allows me to include observations from states that never passed ISRT laws as part of my control group, which is important to weigh against researcher-driven sampling bias (Bertrand and Mullainathan 2003).

E. Sensitivity and Falsification Checks on Threats to Validity

Though more robust than a standard DD strategy, my estimation method remains quasi-experimental, and as such is limited in its ability to support causal findings (Shadish, Cook and Campbell 2002). It is also vulnerable to particular threats of validity that I address through a number of sensitivity and falsification analyses. The greatest

threat to validity comes from introduced, but unavoidable error measuring who is undocumented. In the absence of direct measures, I am forced to proxy undocumented status based on what is known about period of entry, national origin, age, parental origin, and their associations with the likelihood of being undocumented (Judson and Swanson 2011). While residual estimation yields a measure that inadvertently contains documented non-citizens as well as undocumented ones, my proposed measure is the strongest proxy we can identify for undocumented status from governmental data. To test the strength of this proxy, I test differences in educational attainment outcomes among non-Latino foreign-born non-citizens and non-Latino Mexican foreign-born citizens. If I have accurately identified the subgroup which contains undocumented youth, I should not observe policy effects of similar magnitiude, if any at all, targeted toward this population within the foreign-born citizen group.

I also test whether my estimate is truly capturing an ISRT policy effect on the likely undocumented by testing for an effect on non-Latino foreign-born non-citizens and non-Latino Mexican foreign-born citizens. I exclude Latino non-Mexican youth to account for possible spillover effects I might capture were I to include them. If I have truly identified a policy shock unique to Mexican FBNC, I should not observe any effect among either non-Latino group. I include an average state-month measure of Mexican FBNC residents to account for demographic fluctuation and measures of educational attainment for white adults of the previous generation to account for general state education trends.

F. Limitations

In addition to the limitations posed by quasi-experimental analytic methods I discussed above, certain data limitations limit my ability to more fully understand and explain how ISRT impacts educational attainment for likely undocumented youth. First, CPS-MORG data do not distinguish between enrollment in a two-year college and a four-year college. We know that undocumented college students are more likely to be enrolled in two-year rather than four-year colleges (Kurleander 2006). It is possible that ISRT policies influence undocumented student college enrollment behaviors differently by type of college. Because of this limitation, however, I am unable to fully evaluate how ISRT ameliorates these conditions differently depending on the school students attend.

Second, the CPS-MORG only asks enrollment questions of 16-24 year olds. This is a significant limitation because it fails to capture enrollment behaviors of non-traditional college students, among whom are a disproportionate number of undocumented immigrants (Diaz-Strong, Gomez, Luna-Duarte, and Meiners 2011). Because of this limitation, however, I am unable to fully evaluate how IIRIRA might have contributed to this disproportionality or how ISRT might have mitigated against it. Third, the CPS-MORG only asks enrollment questions of 16-24 year olds. This is a significant limitation because it fails to capture enrollment behaviors of non-traditional students, and complicates my ability to truly evaluate how ISRT policies influence delay, stop-out, and start-back enrollment behaviors (Bozick and Miller 2010; Diaz-Strong et al. 2014).

Fourth, related to this, these data do not capture important information such as college type, number of classes enrolled in, tuition assessed, or distance between home

and college that might help explain ISRT policy uptake. Fifth, though the CPS captures more detailed individual- and family-level education and interstate migration measures in its annual October Supplement, these measures are unavailable and un-imputable to its MORG files which contain the best proxy measures for residency status.

Sixth, a final notable limitation is that these are cross-sectional and not longitudinal data. If these were longitudinal data, I might be able to measure individual-level stop-outs and start-backs, and capture the effects of tuition policy on reducing or increasing the time gaps between college attendance with implications for financial aid policy in reducing or increasing the need to stop-out to raise college costs. Despite these limitations, this paper provides important evidence as policy analysts, educators, and lawyers try to understand the impact of access-facilitating laws on undocumented-student college enrollment and degree attainment.

V. Results and Discussion

A. Univariate Analyses

<INSERT TABLE 1 HERE>

To begin my analyses on ISRT's effects on educational outcomes, I perform adjusted Wald tests comparing, by citizenship status, the mean differences before and after state ISRT effective date in 18-21 year-olds having a high-school diploma, 18-24 year-olds being enrolled in college, and 22-30 year olds having an associate's degree or higher and having a bachelor's degree or higher. In Table 1, I report the means and robust standard errors for these outcomes as well as individual-level covariates: age (in years), percentage male, and percentage married by age-cohort panels. I also report, as state-

level characteristics, means and robust standard errors for differences in: monthly unemployment rate, as a measure of monthly changes in labor market conditions; monthly percentages of Mexicans and Mexican foreign-born non-citizens surveyed, as demographic control measures; and monthly percentages of non-Latino whites ages 30-45 with some college education and with a bachelor's degree or higher as a proxy for education trends (see, e.g., Flores and Chapa 2008; Potochnick 2014; Shaw 2016 (for similar descriptive analyses)).

Overall, I observe higher educational attainment among respondents who live in states with ISRT policies (high-school diploma: $p < 0.000$; college enrollment: $p = 0.002$; associate's degree: $p < 0.000$; bachelor's degree: $p = 0.004$). Most of this improvement appears driven by growth in FBNC respondents' attainment. For example, while I observe a 7 percent growth in 18-21 year-old citizens' high-school diploma attainment when ISRT is available (65.32 vs. 70.13, $p = 0.014$), I observe a 20 percent growth in the same outcome among FBNC (40.50 vs. 48.75, $p = 0.002$). I observe greater percentage growth in college enrollment among 18-24 year-old FBNC, 45 percent, even though actual percentage-point growth is smaller (4.47 vs. 6.48, $p < 0.001$). Observed growth in college enrollment among citizens is nonsignificant ($p = 0.354$). Interestingly, observed within-group growth across ISRT in associate's-degree (FBNC: $p = 0.126$; Citizens: $p = 0.856$) and bachelor's-degree attainment is also nonsignificant for both FBNC and citizens (FBNC: $p = 0.487$; Citizens: $p = 0.978$).

In all panels, individual characteristics are generally balanced across ISRT exposure within the FBNC treatment and citizen control group with one notable exception: marital status. Citizens exposed to ISRT are less likely to be married to a

significant degree (18-21: 9.05 vs. 6.44, $p = 0.020$; 18-24: 15.78 vs. 12.51, $p = 0.015$; 22-30: 40.44 vs. 35.48, $p = 0.039$). This might be important for its own sake, and also because citizens are both more likely to be male (18-21: $p = 0.062$; 18-24: $p < 0.001$; 22-30: $p < 0.001$) and less likely to be married (All Panels: $p < 0.001$) than their FBNC peers in each panel. As I discussed in my IIRIRA paper, marriage is known to delay college enrollment for both men and women (Teachman and Polonko 1988). While Teachman and Polonko find that, after a short delay, marriage has a palliative effect on men's likelihood of enrolling and completing college (Teachman and Polonko), most scholars find an association between being married and lower likelihoods of enrollment and degree attainment (Bradburn 2002; Harpe and Kaniuka 2012; Leppel 2002; Negy and Meehan 2003), even if skeptical of a causal relationship (Bozick and DeLuca 2005; Otero, Rivas, and Rivera 2007). Women, particularly those who marry young, are less likely over their lifetime to enroll in college or complete degrees (Deutsch and Schmertz 2011; Hostetler, Sweet, and Moen 2007; Jacobs and King 2002).

Recall, I am observing outcomes resulting from staggered enactments of ISRT laws. Because I am not observing in this ISRT study a static group of states, but rather a dynamic cohort comprised of "before" and "after" ISRT observations, I do not expect covariate balance within the treatment groups. Given this, I observe a number of noteworthy comparisons both within and between the treatment groups, which make a considerable case for controlling for state-level characteristics.

Respondents in both treatment and control groups in all three panels experience higher unemployment rates. The differences between FBNC groups are statistically significant (18-21: 5.96 vs. 6.76, $p = 0.021$; 18-24: 5.65 vs. 6.92, $p = 0.009$; 22-30: 5.75

vs. 7.06, p = 0.017), and are marginally significant for all but one citizen group (18-21: 6.10 vs. 7.39, p = 0.054; 18-24: 6.08 vs. 7.33, p = 0.045; 22-30: 6.07 vs. 7.22, p = 0.068). This may affect the earlier observed differences by residency-status group in educational attainment behavior in two distinct ways. Lower unemployment is generally associated with lower college enrollments, and consequentially lower degree attainment (Fry and Taylor 2013). Compared to citizens in before-ISRT periods, FBNC youth experience lower rates of state unemployment. This might have the effect of dampening the FBNC "pre-treatment" trend toward college, an observation which, particularly if accompanied by other similar imbalances might weigh against a traditional difference-in-differences analysis. Because higher unemployment rates are associated with increased college enrollments (Betts and MacFarland 1995; Hillman and Orians 2013; Pennington, McGinty and Williams 2002), these differences in trend suggest that local labor conditions might be a confounder to my preliminary observations.

Differences in demographic conditions are another important possible confounder for which to account. Between 2000 and 2010, the population of Mexican ancestry in the U.S. grew by 54.1 percent (Ennis, Rios-Vargas and Albert 2011). The differences in growth across ISRT appear to be even greater—by roughly 100 percent ($p < 0.000$). While, as stated earlier, no association has been found between population proportions of Latinos or undocumented residents and ISRT laws (McLendon, Mokher and Flores 2015), other community-level effects on educational attainment might enhance with population growth. Research consistently observes differences in educational opportunities available to Latino youth, generally, and undocumented youth among them when compared to their white, citizen peers. Latino youth, and the undocumented among

them, are more likely to attend poorer resourced schools in terms of facilities, courses available, teacher quality, college counseling, and extra-curricular activities (cf. Gonzales 2010; Perna and Titus 2005; Tornatzky, Cutler, and Lee 2002). Even within these schools, tracking and other resource hoarding might contribute to lower educational attainment rates among undocumented youth (Gonzales 2010).

B. Multivariate Analyses

1. High-School Diploma Attainment

<INSERT TABLE 2 HERE>

As I discussed earlier, and I show in Table 2, I estimate the causal impact of ISRT on educational attainment outcomes using sets of generalized difference-in-difference logistic regression models. Recall, that because of the staggered, dynamic nature of ISRT-policy introduction, all of my models include state-by-year fixed effects to capture general trends that might have affected education attainment choices among all youth of Mexican ancestry instead of a single post-ISRT term. Because of the large time span these data cover, these state-by-year trends might account for important local disruptions like the September 11 attacks, which disproportionately affected the mid-Atlantic and northeastern U.S., and the 2006 landfall and aftermath of Hurricane Katrina, which disproportionately affected Gulf Coast states. These trends also account for local, idiosyncratic spikes in immigration enforcement (see Gonzales, Heredia, and Negrón-Gonzales 2015), shifts in affirmative-action availability, which vary considerably over this time period (Shaw 2016a), and differences by year in social and community services available to this population. As a higher-order term, these effects also account for time-

invariant differences between the states as well as state-invariant differences through the years.

My preliminary Model 1, which I fit accordingly, shows a 23 percent improvement of diploma likelihood due to ISRT (OR: 1.23, $p = 0.006$). To test the robustness of this result to the covariates I described earlier, I add time-varying individual characteristics: age, sex, and marital status to the baseline model in Model 2, and reach a similar finding (OR: 1.30, $p < 0.000$). In Model 3, I add to the baseline model: 1) monthly unemployment rate to account for state-specific economic trends; 2) the percentage of white non-Latino adults ages 30-45 who report having at least some college education to control for state-specific education trends; 3) and the percentage of persons in a state who report being Mexican FBNCs in a given month to measure state migration trends. These results are robust to the inclusion of these time-varying state characteristics (OR: 1.23, $p = 0.005$), as well as to the inclusion in Model 4 of both individual and state time-varying characteristics (OR: 1.30, $p < 0.000$). Because of the seasonal nature of educational enrollments and credentialing, I add month fixed effects in Model 5 to absorb the impact of seasonal differences that might be confounding these results (OR: 1.22, $p < 0.000$), and find that ISRT laws improve likely undocumented students' high-school diploma attainment by 22 percent.

As I show in Appendix 2, these findings are stable in both magnitude and statistical significance to the different groupings of states I present in Figure 2 (see Shaw 2016b): in Model 6 to the 10 states where Mexican immigrants traditionally settle: Arizona, California, Colorado, Florida, Illinois, Nevada, New Jersey, New York, or Texas (OR: 1.21 $p < 0.001$); in Model 8 to the 10 states with the highest population

percentages of Mexican immigrants in 1996, when IIRIRA was passed: California, Colorado, Florida, Idaho, Illinois, Nevada, New York, Oregon, Texas, and Utah (OR: 1.24, $p < 0.001$); in Model 9 to the top-10 immigrant-destination states by population proportion ten years later in 2006: Arizona, California, Florida, Georgia, Maryland, Nevada, New Jersey, New Mexico, Texas, and Washington (OR: 1.21, $p < 0.000$). and in Model 10 to the 10 states which passed in-state-resident-tuition (ISRT) laws within 10 years of IIRIRA's passage: California, Illinois, Kansas, Nebraska, New Mexico, New York, Oklahoma, Texas, Utah, and Washington (OR: 1.20, $p = 0.001$).

I conduct two additional sets of analyses to explore whether my observed effect is spurious. First, I evaluated whether there was any positive association between the law's enactment and high-school diploma attainment for non-Latino Black and non-Latino white youth, comparing diploma odds for each group to all other non-Latino youth. I observe significant findings for both sets of youth. One explanation for this result could emerge from the construction of the law itself. In order to avail undocumented students of ISRT, lawmakers had to make ISRT available to citizens and legal residents who otherwise would not have been eligible.

I also evaluate whether ISRT had an anticipated effect, by setting a placebo shock one year before policy enactment, and delayed effects, by setting placebo shocks one, two, and three years after policy enactment. I observe a larger policy effect on high-school diploma receipt one year before enactment (OR: 1.40, $p = 0.035$) than at policy enactment (Model 5 OR: 1.22, $p < 0.000$), which suggests that likely undocumented high-school students, anticipating greater college access, respond to policy passage through persistence to high-school graduation.

Consistent with Bozick and Miller (2014) and Potochnick (2014)'s findings that ISRT laws reduce high-school dropout, these results support a conclusion that ISRT causes high-school students to persist to graduation. Undocumented students, particularly those who live in areas with higher concentrations of fellow undocumented students or who have access to social and information networks, are generally aware about ISRT laws (see Abrego 2006). Similar to the high-school students identify, who were easily able to list Georgia HOPE scholarship eligibility requirements when prompted (Dynarski 2000), undocumented students are keenly aware of both ISRT laws and the potential impact on their lives. In her qualitative interview study of undocumented California students before and after its ISRT law passed, Abrego (2006) observes not only a savvy awareness among her student interviewees, but also an assumption of status among hopeful college students. Observing one of her participants, Abrego writes of her shame, social isolation, and acute sense of stigma about her residency status before the law. Reflecting on an interview conducted after the law's passage, Abrego reflects on her participant's increased engagement with teachers, proactively seeking help with her college applications and enrollments, and even disclosure of her residency status to her friends. Following the California law's passage, Abrego notes that all of her respondents who were familiar with the law recounted stories of affirming interactions with staff—in both high school and college—during which they "came out" by adopting the status moniker, "AB 540 Student" after the bill's reference number, understanding the law as a legitimizing agent in their negotiations and engagements with authority and peers.

To the extent ISRT-induced shifts toward self-affirming self-efficacious behavior is generalizable, it may also offer a related explanation for my findings, particularly those

indicating a policy effect three years after enactment (OR: 1.28, p = 0.006). Generally, undocumented students are unlikely to ask for academic assistance or engage with authority figures with whom they do not have a personal trust relationship (C. Suárez-Orozco et al. 2015). Those who are able to develop positive, trusting relationships with school counselors, college staff, and support networks have better odds of persisting through to the next educational stage than those that remain isolated, as many do (Gonzales 2010). In high schools, these relationships can be quite consequential, particularly in the low-resource schools undocumented students are likely to attend. Due to the scarcity of higher-level courses and other resources needed to successfully transition students to college, school officials, guidance counselors in particular, are often the gatekeepers to higher education (Gonzales).

Being "positively tracked" operates as a feedback loop of sorts, particularly in large urban high schools. Being placed well in school is associated with smaller class sizes, more challenging academic work, more frequent and higher quality interaction with teachers—who then are more likely to go above and beyond to assist students in whom they have a stronger investment. Importantly, this tracking deepens these students' well of confidence in their ability and in their worth, which is often desperately needed to weather the shocks imposed by "illegality" upon adulthood (Gonzales 2010).

By reducing college sticker price, ISRT laws also incentivize continued investment in high-school education with the promise of lower-than-expected higher-education costs and consequently greater likelihood of access. In separate studies of the Georgia HOPE scholarship, Dynarski (2004) and Cornwell, Mustard and Sridhar (2006) found improvements in high-school graduation rates associated with the scholarship.

While HOPE's estimated effect might have been due to the requirement that a graduating high-school senior obtain a 3.0 grade-point average, and not necessarily the promise of lower tuition cost writ large, these studies and others support the idea that with information of lower college tuition cost, aspiring high-school students will respond by improved academic performance. There is thin evidence of a possible gender and marital-status differences in ISRT effect on high-school diploma attainment.

2. College Enrollment

<INSERT TABLE 3 HERE>

Interestingly, given the observed inducement to graduate high school, ISRT laws appear to have a weak effect on college enrollment. At first glance, an association seems likely. In my preliminary Model 1 on Table 3, I show a 26 percent improvement in enrollments in after-ISRT periods (OR: 1.26, p = 0.004), similar to my observed effects on high-school graduation. With the inclusion of individual-level covariates in Model 2, this result, though of similar magnitude, trends toward marginal significance (OR: 1.22, p = 0.063). While a model including just state-level covariates is statistically significant (Model 3 OR: 1.27, p = 0.004), a model containing both sets of covariates returns to marginal significance (Model 4 OR: 1.23, p = 0.053) where it remains upon the inclusion of month fixed effects (Model 5 OR: 1.24, p = 0.069).

This effect becomes entirely non-significant when I limit my analytic sample to the groups I display in Figure 2: in Model 7 to the traditional immigrant destination states (p = 0.233), in Model 8 to states with high concentrations of Mexican immigrants in 1996 (p = 0.212), in Model 9 to immigrant-destination states in 2006 (p = 0.252), and in Model 10 to early-passing ISRT states (p = 0.201).

While I don't find an immediate affect among likely undocumented youth, I find positive effects on enrollment one (OR: 1.27, $p < 0.001$) and two years (OR: 1.21, $p = 0.001$) after policy enactment, with effects tapering—and showing signs of reversal by the third year (OR: 0.80, $p = 0.050$). Importantly, I do not observe any placebo shock one year before enactment, which suggests that any effects ISRT might have on college enrollment are related to the actual cost intervention, and are delayed ($p = 0.451$). Surprisingly, I observe an immediate ISRT enrollment effect for single women, with women being 40 percent more likely to enroll in college immediately after the law is effective (Model 15 OR: 1.404, $p = 0.015$).

An elegant explanation might simply be information delays and roadblocks related to difficulties in policy uptake. While knowledge *about* these laws is fairly diffuse (see Abrego 2006), information on *how* to take advantage of them and technical savvy in meeting statutory requirements, which vary by state, is harder to find, and even harder for students unfamiliar with higher-education bureaucracies to successfully complete on their own. The genius behind the Georgia HOPE scholarship program is the ease with which students confirm their eligibility and access funding. As Dynarski (2000) notes, high schools submit the bulk of academic information and students and families complete at the most a four-page supplemental form. The general know-how required to complete a HOPE scholarship application is low. Nevertheless, college officials are available to check for and correct errors.

Compare the simplicity of Georgia HOPE to the procedures required to take advantage of California's ISRT laws. Though there is a standard form affidavit that a prospective student must complete in order to receive an eligibility determination, there

are three different procedures one must follow depending on whether one is seeking to attend a community college, University of California (UC) or California State University (ab540.com n.d.). In addition to providing one of a list of specified verifying documents, each institution is allowed to request additional documentation. To complicate things further, UC institutions each maintain their own application and affidavit forms, and deadlines, require a student to have been admitted and filed statements of intent to register and of legal residence (ab540.com).

For undocumented students who manage to navigate unclear college enrollment procedures (Diaz-Strong et al. 2010), their reward is often engagement with new bureaucracies that govern tuition. Gonzales (2016) documents how inefficient bureaucracies can be when staffed by uncaring personnel who often give students contradictory, if not wrong information affect policy uptake. Many support personnel are simply unfamiliar with the byzantine rules, timing structures, and requirements they have as an institution to secure student eligibility (Morales, Herrera and Murry 2011). When enrollment is predicated on affordability and meeting ISRT requirements in a timely manner is the only chance one has at meeting the cost of attendance, information and support become critical components of one's success. Bettinger et al.'s (2012) study on FAFSA uptake is instructive in this regard. In their study, the authors found that students from families who received personal assistance and simplified information were 29 percent more likely to have completed two years of college (28 vs. 36 percent), while students whose families received only information showed no improvement in college participation.

3. Associate's Degree Attainment

<INSERT TABLE 4 HERE>

Likely undocumented students who manage to successfully navigate unclear college enrollment and tuition bureaucracies do not persist to degree completion in any greater number after ISRT (Model 1: p = 0.515). Controlling for time-varying individual characteristics (Model 2: p = 0.606), time-varying state characteristics (Model 3: p = 0.496), both sets of time-varying characteristics (Model 4: p = 0.583), and month fixed effects (Model 5: 0.675) does not adjust this result.

I also observe null effect findings when I limit my analytic sample to certain groups of states: traditional immigrant (Model 6: p = 0.713) and top-10 destination states from 1996 (Model 7: p = 0.735), top-10 destination states from 2006 (Model 8: p = 0.975), and early-ISRT states (Model 9: p = 0.281).

Further, I find neither a meaningful spillover effect nor a delayed effect that would help explain poor policy take up.

The absence of degree-attainment results suggests a diminution of ISRT's effect at later stages in the educational attainment trajectory, a hypothesis I find support for below in my analyses of bachelor's-degree-holding odds. However, when I examine associate's-degree outcomes by gender and marital status, I reach different results. For single men, ISRT does improve the odds of holding an associate's degree by nearly 80 percent (Model 14 OR: 1.34, p = 0.048). Overall, these findings begin to raise the specter that tuition subsidies alone are insufficient to meet the out-of-pocket demands on undocumented students in the absence of federal financial aid. Again, fifty-four percent of community-college students in Suárez-Orozco et al.'s (2015) study report paying for at

least half their tuition out-of-pocket. The authors also report that community college students are more likely than their four-year-college peers to attend school part time, work while enrolled, and are less likely to live on campus. Gonzales (2016) attributes similar behaviors almost entirely to attempts by college goers to meet costs. "Stopping out," the temporary withdrawal from college with the intent to return, becomes an all too salient phenomenon. Associated with higher rates of stopping out are lower rates of degree completion both in two- and four-year colleges (Terriquez 2015).

4. Bachelor's Degree Attainment

<INSERT TABLE 5 HERE>

Generally, ISRT subsidies are thought to encourage students, and low-income students particularly, away from attending two-year colleges and toward attending four-year colleges where they might go on to receive a bachelor's degree (Long 2004). One might expect ISRT to have a more profound influence on bachelor's-degree attainment than these somewhat marginal findings suggest. However, as I show in Table 5, the gradual decline in ISRT effect appears to extend to bachelor's-degree attainment.

As I show in Appendix 5, these findings are robust to states being analyzed. Similar to associate's-degree attainment, I show no spillover effect or delay in effect. I also observe no heterogeneity in this null effect. While, recall, measurement error may be intervening to attenuate a true significant finding, it is also possible that these results support a finding that ISRT is limited in its reach (Conger and Chelman 2011). If one is to take these findings at face value, one could understand undocumented students as being more acutely sensitive to tuition price in ways their peers are not. The aforementioned price differential amounts to one third of an undocumented family's

income, which is already 60 percent less than the average American family's income. Perhaps one-dollar difference simply means more to these students. In general, most undocumented young adults work long hours at low paying jobs (that have relatively few protections due to their status) just to meet living expenses (Perez 2010). To meet college costs above and beyond living expenses, students work more hours, attend school part-time, or compromise their academic performance and progress (Diaz-Strong, Gómez, Luna-Duarte, and Meiners 2010; cf. Suárez-Orozco et al., 2015). A rational student might expect that a reduction in tuition costs might lead to fewer work hours, more time to study, and better quality of life, and choose to enroll accordingly.

A related explanation would understand the difference in effects as exposing how tuition sensitive a population might be in the absence of federal financial aid. Citizen and legal residents generally have access to financial aid, and through such aid are more likely to be able to pay assessed tuition costs than the undocumented. If so, the lack of tuition effect does not necessarily mean insensitivity to cost, but it could mean that tuition costs do not substantially exceed aid availability for documented students who were otherwise ineligible for in-state-resident tuition. Or, one might conclude that without aid, ISRT does not improve college attainment among the undocumented.

A third explanation takes an opposing view point, and questions the benefit of a college education for an undocumented student who cannot use his education to improve his social or financial situation (Gonzales 2016). An adult facing mounting costs of all types and no returns from college might have a different mindset about continuing education than a teenager who has not fully lived a liminal existence. Given the large

shadow IIRIRA casts to frame the lives of on undocumented people, maybe I am not observing significant changes in degree attainment because there are none.

Though I cannot explore this theory using these data, research suggests that these findings are not the result of differences in regard for higher education. If anything research suggests the opposite. Diaz-Strong et al. (2010) and Suárez-Orozco et al. (2015) separately identify a particularly resilient subset of undocumented students: the likely cohort driving the observed improvement in enrollments (Gonzales 2009; 2016). These students, like their citizen and resident counterparts, believe that attending college and attaining a bachelor's degree will yield economic stability and socioeconomic mobility. Many attempt college enrollment only to face financial difficulties, usually in the form of unexpected additional costs to education or family stresses, that cause them to increase their work hours, reduce enrollment to part-time, stop-out periodically to raise money to continue their education, or stop entirely (Frum, 2007; McDonough and Calderone 2006).

VI. Implications and Conclusion

Using the same dataset as most national studies on ISRT, I examine ISRTs effect on high-school completion and college enrollment using a generalized difference-in-difference methodology. While my high-school completion findings are in line with previous research (see Bozick and Miller 2014; Potochnick 2014), my associate's-degree attainment findings suggest that ISRT is not having the impact scholars believe it to have (contra Flores 2010). Taken together, my finding that undocumented students respond meaningfully to the passage of ISRT laws through improved high-school performance, but are unable to convert this enthusiasm to college-degree attainment is a mixed bag. On

the one hand, they reinforce that tuition costs matter, particularly for low income students and their families (cf. Long 2004). By the same token, they lay bare a matrix of realities, either directly or indirectly involving financing education, that continue to keep college at arm's length from most undocumented students. By outlining the limits of tuition-only remedies, my results imply that without access to the largest financial aid system in this country, undocumented students simply do not have a chance at attending and graduating from college in meaningful numbers.

I conclude that tuition subsidies made available through ISRT laws, though helpful, are woefully deficient operating alone to cure the harm inured by this generation of undocumented young people. If the goal is to remedy the effects that tuition barriers cause on undocumented-student access, then these have been largely successful. Most states with ISRT have taken this approach: create an avenue to obtain tuition subsidy, and let the market sort out the remainder. If, however, the goal is to facilitate meaningful access to higher education, more needs to be done. Some states like California, Minnesota, New Mexico, Texas, and Washington Minnesota have created state financial-aid benefit programs to try to make up some of the aid differential. However, the need is too great and diffuse, and the reach of state-based programs and private philanthropies too short. As literature underscoring the importance of federal financial aid to aspiring students has often reminded us, if we want to improve the rates of college attendance, and more importantly completion, we must explore how to expand federal financial aid opportunities to this population. These results, thus, implicate why the DREAM Act or similar legislation is needed at the federal level.

Without a college education, many undocumented adults in 2015 find themselves in the same position that elementary and secondary school children were in when *Plyler* was decided in 1982. As a nation, we should take heed of the *Plyler* Court's concern that without access to education, we are creating in undocumented residents a permanent underclass in a manner inconsistent with the spirit of the U.S. Constitution. Worse, in so doing, lawmakers are stifling the potential of thousands of young people who have grown up in this country, been educated in U.S. schools, and want to build a secure life for themselves and their families. While these observations might be felt more acutely in the quickly growing Latino community, they are not uniquely felt there.

Despite these findings of clearly, demonstrated harm to an identifiable group of people, the law offers a complicated response. Foremost, college is not generally available to all people in the same manner that elementary and secondary education is. There are many people who are priced out of college either because the tuition costs are too high or the aid available to them is too low. The *Plyler* analysis requiring states to admit undocumented youth to K-12 education did so based on grammar schools being free for all residents. In addition, the undocumented are not a suspect class for which the law provides special protections, and is likely to remain so. One does not have a federally-recognized fundamental right, in the Supreme Court's eyes, to a higher education. And so, the ability to address this law through the courts is complex, and worth future exploration. In an earlier paper, I proposed extending *Plyler v. Doe* (1982) by either (1) acknowledging that a post-secondary education has replaced a high-school education as the "basic education" spoken of in *Plyler*; or (2) accepting the Court's

invitation in *Papasan v. Allain* (1986) to establish education access as a fundamental right (Shaw 2016).

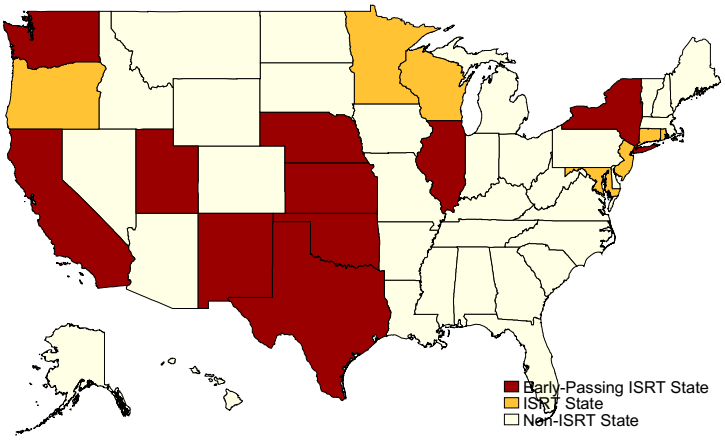
Ultimately, these findings raise profound implications for undocumented students and for the United States as our demographics shift. Consistent with prior research, I observe likely undocumented residents reporting college enrollment despite burdens posed by IIRIRA. In addition to future research, which should explore what motivates undocumented students who decide not to attend college as well as how those who do decide to attend manage to persist, perhaps we need to rethink provisions of IIRIRA that operate best to foreclose legitimate opportunities to education and work.

Figure 1. Timeline of significant events in undocumented-student college access.

Year	Facilitative Event	Restrictive Event
1982	*Plyler v. Doe*: States are required to allow undocumented children access to public K-12 education	*Toll v. Moreno*: *Plyler* not extended to higher ed.
August 22, 1996		Congress passes Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA); President Clinton signs.
September 30, 1996		Congress passes Immigrant Reform and Immigrant Responsibility Act of 1996 (IIRIRA); President Clinton signs.
July 1, 1997		PRWORA effective: Undocumented ineligible for federal financial aid
July 1, 1998		IIRIRA effective: States cannot offer in-state-resident tuition (ISRT) to undocumented unless same offer is made to citizens and legal residents; Colleges must submit proof of financial aid applicant's lawful immigration status
1999		
2000		
June 16, 2001	Texas passes first state to pass in-state-resident-tuition (ISRT) law; TX ISRT law immediately effective	
2002	ISRT effective: CA, UT	
2003	ISRT effective: OK, IL, WA, NY	
2004	ISRT effective: KS	
2005	ISRT and SFAB effective: NM	
2006	ISRT effective: NE	ISRT banned: AZ, CO
2007		ISRT repealed: OK
2008		ISRT banned: GA Undocumented students barred: SC, NC (community colleges, "CC," only)
2009	Undocumented students allowed: NC (community colleges only) ISRT effective: WI	
2010		
2011	ISRT effective: CT	ISRT repealed: WI Undocumented students barred: AL
2012	ISRT allowed: RI ISRT effective: MD (CC only)	
2013	ISRT effective: MN, OR, NJ SFAB effective: CA, MN	

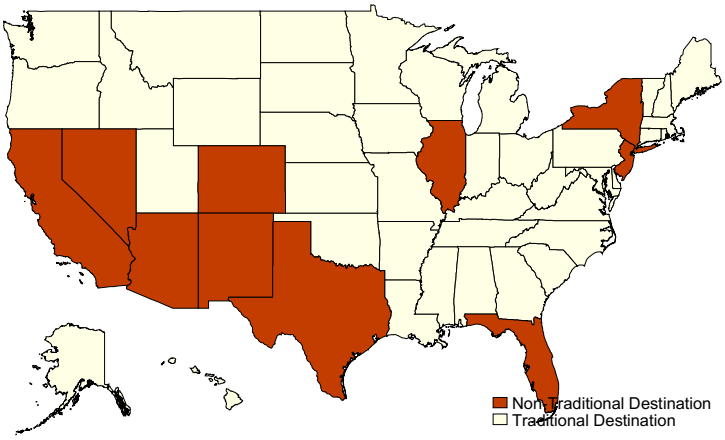
Figure 2. States by In-State-Resident-Tuition (ISRT) Law and Immigration Histories.

Map 1: ISRT States (enactment year in parentheses)[22]



Early-Passers	Late-Passers
Texas (2001)	Wisconsin (2009)[23]
California (2002)	Connecticut (2011)
Utah (2002)	Rhode Island (2012)
Oklahoma (2003)[24]	Maryland (2012)
Illinois (2003)	Minnesota (2013)
Washington (2003)	Oregon (2013)
New York (2003)	New Jersey (2013)
Kansas (2004)	
New Mexico (2005)	
Nebraska (2006)	

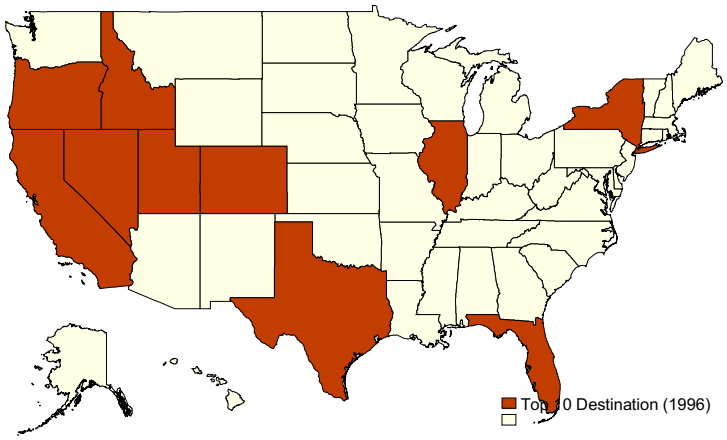
Map 2: Traditional Destination States for Mexican Immigrants[25]



Arizona	Nevada
California	New Jersey
Colorado	New Mexico
Florida	New York
Illinois	Texas

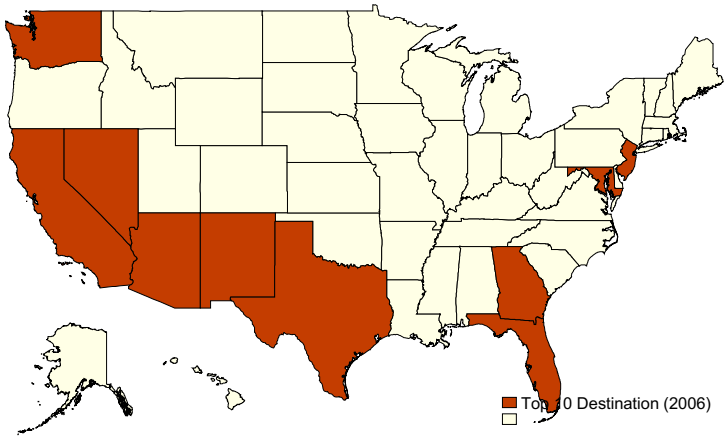
[22] See Appendix 1 for additional details.
[23] Repealed in 2011 (see Appendix 1).
[24] Repealed in 2007 (see Appendix 1).
[25] See Durand, Massey, & Charvet, 2000; Passel, 2005.

Map 3: Top-10 Mexican Immigrant Destination States in 1996 (population share in parentheses)[26]



California (46.6%)	Utah (8.1%)
Texas (16.7%)	Idaho (7.6%)
Nevada (12.8%)	Oregon (6.9%)
Florida (10.4%)	Illinois (6.6%)
New York (9.0%)	Colorado (6.4%)

Map 4: Top-10 Mexican Immigrant Destination States in 2006 (population share in parentheses)[27]



Nevada (7.2%)	Maryland (4.6%)*
California (6.8%)*	Washington (4.5%)*
Texas (6.7%)*	Florida (4.5%)
New Jersey (6.2%)*	Georgia (4.4%)
Arizona (6.0%)	New Mexico (4.3%)*

[26] Durand, Massey and Charvet (2000).
[27] Passel and Cohn (2011). Asterisk denotes ISRT state as of 2013.

Table 1. Summary Statistics.

	Foreign-Born Non-Citizen					U.S. Citizens				
	Before ISRT		After ISRT			Before ISRT		After ISRT		
	Mean	SE	Mean	SE		Mean	SE	Mean	SE	
Age 18-21										
Outcome										
High-School Diploma (%)	40.50	2.28	48.75	1.43	**	65.32	1.21	70.13	1.92	*
Covariates										
Individual Characteristics										
Age (years)	19.72	4.02	19.65	1.90	+	19.43	2.05	19.42	1.51	
Male (%)	55.76	1.25	57.01	1.05		52.30	0.84	50.38	1.46	
Married (%)	20.05	1.69	18.81	0.60		9.05	1.47	6.44	1.04	*
State Characteristics										
Unemployment Rate (%)	5.69	0.15	6.76	0.43	*	6.10	0.28	7.39	0.64	+
Mexican (%)	11.89	2.42	25.51	2.45	***	14.40	2.50	26.06	2.51	*
Mexican FBNC (%)	5.10	0.96	9.01	0.94	***	4.85	0.97	8.61	0.96	*
White, HS Diploma (30-45, %)	50.79	1.11	57.06	2.28	**	52.23	0.90	57.16	2.18	** *
# of observations	7089		4942			14787		15490		
Age 18-24										
Outcome										
Enrolled in College (%)	4.47	0.36	6.48	0.16	** *	8.20	0.47	8.78	0.41	
Covariates										
Individual Characteristics										
Age (years)	21.39	0.06	21.29	0.06		20.84	4.05	20.83	2.10	
Male (%)	59.99	1.06	58.12	0.86		50.92	0.79	49.36	0.48	
Married (%)	30.47	1.31	28.95	1.51		15.78	1.94	12.51	1.66	*

State Characteristics										
Unemployment Rate (%)	5.65	0.14	6.92	0.46	**	6.08	0.26	7.33	0.60	*
Mexican (%)	11.76	2.38	24.88	2.70	***	14.22	2.47	25.83	2.61	***
Mexican FBNC (%)	5.02	0.93	8.79	1.02	***	4.80	0.96	8.54	1.00	***
White, some college (30-45, %)	51.04	0.98	57.19	2.19	**	52.18	0.93	57.17	2.21	*
# of observations	14121		9571			23817		25255		
Age 22-30										
Outcome										
Associate's Degree or higher (%)	4.92	0.55	6.02	0.49		21.29	2.64	21.79	0.81	** *
Bachelor's Degree or higher (%)	3.02	0.32	3.62	0.87		12.65	1.81	12.70	0.87	*
Covariates										
Individual Characteristics										
Age (years)	26.14	0.56	26.39	0.09	***	25.97	0.05	25.84	0.07	
Male (%)	59.95	1.35	58.80	0.35		49.97	0.90	49.93	0.52	
Married (%)	56.17	1.80	55.24	1.59		40.44	1.57	35.48	2.23	*
State Characteristics										
Unemployment Rate (%)	5.75	0.16	7.06	0.48	*	6.07	0.25	7.22	0.58	+
Mexican (%)	11.70	2.48	24.80	2.74	***	13.81	2.39	25.59	2.78	***
Mexican FBNC (%)	5.06	1.02	8.80	1.10	***	4.64	0.97	8.43	1.03	***
White, some college (30-45, %)	51.29	1.00	57.14	2.04	***	52.26	0.92	57.09	2.22	*
White, BA + (30-45, %)	19.08	0.64	22.09	0.94	***	19.61	0.59	22.68	1.08	**
# of observations	9503		7921			9830		7153		

Notes: Robust standard errors. $^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$ indicate statistical significance for mean comparisons of Before ISRT and after IIRIRA observations using adjusted Wald tests.

Table 2. Main Effects, High-School-Diploma Attainment.

	(1)	(2)	(3)	(4)	(5)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ Month FE
FBNC after ISRT	1.231**	1.300***	1.228**	1.298***	1.226***
(1 = yes; 0 = no)	(0.089)	(0.087)	(0.085)	(0.087)	(0.056)
FBNC	0.397***	0.340***	0.401***	0.341***	0.343***
(1 = yes; 0 = no)	(0.039)	(0.038)	(0.041)	(0.039)	(0.037)
Age (years)		1.832***		1.831***	1.843***
		(0.058)		(0.057)	(0.050)
Male		0.681***		0.681***	0.680***
(1 = yes; 0 = no)		(0.021)		(0.022)	(0.022)
Married		0.469***		0.468***	0.475***
(1 = yes; 0 = no)		(0.021)		(0.021)	(0.021)
Unemployment Rate (%)			0.938+	0.962	0.936*
			(0.035)	(0.028)	(0.031)
30-45 Whites w/ some college			1.884+	2.029*	1.366
(1 = yes; 0 = no)			(0.600)	(0.697)	(0.393)
Mexican FBNC in a state (%)			0.961***	0.975	0.978
			(0.011)	(0.017)	(0.022)
Constant	3.876***	0.000***	5.009***	0.000***	0.000***
	(0.042)	(0.000)	(1.530)	(0.000)	(0.000)
Observations	35999	35999	35999	35999	35999

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Table 3. Main Effects, College Enrollment.

	(1)	(2)	(3)	(4)	(5)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ Month FE
FBNC after ISRT (1 = yes; 0 = no)	1.259** (0.097)	1.221+ (0.129)	1.270** (0.100)	1.225+ (0.126)	1.241+ (0.144)
FBNC (1 = yes; 0 = no)	0.653*** (0.065)	1.064 (0.098)	0.644*** (0.066)	1.063 (0.099)	1.049 (0.106)
Age (years)		0.377*** (0.017)		0.377*** (0.016)	0.373*** (0.018)
Male (1 = yes; 0 = no)		1.198+ (0.113)		1.198+ (0.112)	1.195+ (0.117)
Married (1 = yes; 0 = no)		0.474** (0.125)		0.475** (0.123)	0.462** (0.121)
Unemployment Rate (%)			0.986 (0.046)	0.936 (0.088)	0.963 (0.098)
30-45 Whites w/ some college (1 = yes; 0 = no)			0.794 (0.238)	0.792 (0.282)	1.012 (0.341)
Mexican FBNC in a state (%)			1.037*** (0.006)	1.002 (0.011)	1.019+ (0.011)
Constant	0.038*** (0.000)	9.78×10^{7}*** (8.21×10^{6})	0.036*** (0.011)	1.51×10^{7}*** (8.05×10^{6})	1.97×10^{7}*** (1.20×10^{6})
Observations	59279	59279	59279	59279	59279

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Table 4. Main Effects, Associate's-Degree Attainment.

	(1)	(2)	(3)	(4)	(5)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ Month FE
FBNC after ISRT (1 = yes; 0 = no)	1.130	1.112	1.134	1.118	1.094
	(0.210)	(0.227)	(0.208)	(0.226)	(0.233)
FBNC (1 = yes; 0 = no)	0.175***	0.176***	0.174***	0.175***	0.177***
	(0.032)	(0.036)	(0.031)	(0.035)	(0.037)
Age (years)		1.120***		1.120***	1.120***
		(0.011)		(0.011)	(0.011)
Male (1 = yes; 0 = no)		0.638***		0.637***	0.638***
		(0.059)		(0.059)	(0.058)
Married (1 = yes; 0 = no)		0.793**		0.792**	0.798**
		(0.064)		(0.064)	(0.063)
Unemployment Rate (%)			0.949	0.940	0.930
			(0.063)	(0.054)	(0.047)
30-45 Whites w/ some college (1 = yes; 0 = no)			1.219+	1.313*	1.228+
			(0.138)	(0.136)	(0.127)
Mexican FBNC in a state (%)			1.003	1.005	1.004
			(0.024)	(0.022)	(0.021)
Constant	0.129***	0.009***	0.147***	0.011***	0.010***
	(0.002)	(0.002)	(0.072)	(0.005)	(0.004)
Observations	78167	78167	78167	78167	78167

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Table 5. Main Effects, Bachelor's-Degree Attainment.

	(1)	(2)	(3)	(4)	(5)
	Baseline	+ Indiv. Covariates	+ State Covariates	+ All Covariates	+ Month FE
FBNC after IIRIRA (1 = yes; 0 = no)	1.196 (0.435)	1.182 (0.473)	1.192 (0.429)	1.179 (0.469)	1.156 (0.467)
FBNC (1 = yes; 0 = no)	0.162*** (0.040)	0.162*** (0.045)	0.163*** (0.038)	0.162*** (0.044)	0.163*** (0.046)
Age (years)		1.161*** (0.011)		1.161*** (0.011)	1.160*** (0.011)
Male (1 = yes; 0 = no)		0.634*** (0.066)		0.634*** (0.065)	0.635*** (0.062)
Married (1 = yes; 0 = no)		0.744* (0.095)		0.745* (0.095)	0.751* (0.090)
Unemployment Rate (%)			1.000 (0.097)	0.991 (0.089)	0.978 (0.084)
30-45 Whites w/ Bachelor's + (1 = yes; 0 = no)			0.746 (0.295)	0.801 (0.301)	0.788 (0.224)
Mexican FBNC in a state (%)			0.992 (0.033)	0.994 (0.030)	0.989 (0.027)
Constant	0.066*** (0.001)	0.002*** (0.000)	0.074*** (0.045)	0.002*** (0.002)	0.002*** (0.001)
Observations	77399	77399	77399	77399	77399

$^{+}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 2A. Sensitivity and Falsification Analyses, High-School-Diploma Attainment.

	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Traditional States	Top 10 States (1996)	Top 10 States (1996)	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	-1 Year	+1 Year	+2 Years	+3 Years
FBNC after ISRT (1 = yes; 0 = no)	1.214***	1.244***	1.214***	1.199**	0.931	0.836	1.403*	1.162	1.155	1.279**
	(0.046)	(0.048)	(0.037)	(0.047)	(0.142)	(0.136)	(0.220)	(0.123)	(0.108)	(0.110)
FBNC (1 = yes; 0 = no)	0.344***	0.330***	0.334***	0.351***	0.318***	1.387+	0.291***	0.361***	0.364***	0.348***
	(0.045)	(0.040)	(0.042)	(0.047)	(0.059)	(0.238)	(0.026)	(0.040)	(0.044)	(0.038)
Age (years)	1.863***	1.858***	1.836***	1.825***	1.310***	1.960***	1.842***	1.843***	1.843***	1.843***
	(0.060)	(0.060)	(0.056)	(0.049)	(0.024)	(0.058)	(0.050)	(0.049)	(0.050)	(0.050)
Male (1 = yes; 0 = no)	0.676***	0.661***	0.671***	0.671***	0.705***	0.668***	0.681***	0.681***	0.680***	0.680***
	(0.023)	(0.020)	(0.023)	(0.022)	(0.021)	(0.052)	(0.021)	(0.022)	(0.022)	(0.022)
Married (1 = yes; 0 = no)	0.456***	0.456***	0.453***	0.455***	0.543***	0.451***	0.476***	0.475***	0.475***	0.475***
	(0.016)	(0.017)	(0.018)	(0.018)	(0.021)	(0.028)	(0.021)	(0.021)	(0.021)	(0.020)
Unemployment Rate (%)	0.929+	0.936+	0.945	0.905**	0.957	0.913*	0.938+	0.938+	0.938+	0.938+
	(0.033)	(0.033)	(0.034)	(0.027)	(0.038)	(0.032)	(0.030)	(0.030)	(0.030)	(0.030)
Whites 30-45, some college (1 = yes; 0 = no)	1.243	1.448	1.185	1.371	1.092	0.827	1.363	1.363	1.361	1.369
	(0.449)	(0.604)	(0.449)	(0.605)	(0.243)	(0.136)	(0.392)	(0.390)	(0.388)	(0.387)
Mexican FBNC in a state (%)	0.990	1.008	0.987	1.000	0.973	0.964+	0.978	0.978	0.978	0.978
	(0.023)	(0.012)	(0.023)	(0.017)	(0.017)	(0.019)	(0.021)	(0.022)	(0.022)	(0.022)
Constant	0.000***	0.000***	0.000***	0.000***	0.020***	0.000***	0.000***	0.000***	0.000***	0.000***

	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	27983	26434	25217	24818	83540	40999	35999	35999	35999	35999

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 2B. Heterogeneity Effects, High-School-Diploma Attainment.

	(16)	(17)	(18)	(19)
	Men	Women	Married	Unmarried
FBNC after ISRT	1.269*	1.236*	1.265	1.228*
(1 = yes; 0 = no)	(0.139)	(0.120)	(0.387)	(0.108)
FBNC	0.336***	0.325***	0.282**	0.335***
(1 = yes; 0 = no)	(0.034)	(0.050)	(0.107)	(0.024)
Age (years)	1.823***	1.910***	1.374***	1.908***
	(0.054)	(0.102)	(0.059)	(0.043)
Male			0.823**	0.664***
(1 = yes; 0 = no)			(0.051)	(0.026)
Married	0.533***	0.420***		
(1 = yes; 0 = no)	(0.031)	(0.034)		
Unemployment Rate (%)	1.025	0.886*	1.084	0.917+
	(0.090)	(0.048)	(0.123)	(0.041)
30-45 Whites w/ some college	1.283	1.559	1.761	1.296
(1 = yes; 0 = no)	(0.436)	(0.485)	(1.243)	(0.307)
Mexican FBNC in a state (%)	0.971	0.988	1.060***	0.974
	(0.024)	(0.016)	(0.016)	(0.022)
Constant	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	18175	17368	2971	32562

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 3. Sensitivity and Falsification Analyses, College Enrollment.

	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Traditional States	Top 10 States (1996)	Top 10 States (1996)	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	-1 Year	+1 Year	+2 Years	+3 Years
FBNC after ISRT (1 = yes; 0 = no)	1.141	1.133	1.166	1.195	1.157	0.720^{+}	1.125	1.270***	1.209**	0.797^{+}
	(0.118)	(0.106)	(0.146)	(0.154)	(0.231)	(0.128)	(0.175)	(0.075)	(0.067)	(0.090)
FBNC (1 = yes; 0 = no)	1.124	1.152^{+}	1.107	1.089	1.057	1.076	1.108	1.047	1.091^{+}	1.403***
	(0.098)	(0.080)	(0.119)	(0.116)	(0.135)	(0.183)	(0.176)	(0.062)	(0.055)	(0.119)
Age (years)	0.377***	0.375***	0.377***	0.380***	0.390***	0.372***	0.373***	0.373***	0.373***	0.373***
	(0.020)	(0.022)	(0.022)	(0.022)	(0.027)	(0.023)	(0.017)	(0.018)	(0.018)	(0.017)
Male (1 = yes; 0 = no)	1.189	1.229	1.218	1.263^{+}	1.155	1.217^{+}	1.196^{+}	1.196^{+}	1.195^{+}	1.197^{+}
	(0.137)	(0.142)	(0.151)	(0.153)	(0.124)	(0.120)	(0.117)	(0.117)	(0.117)	(0.117)
Married (1 = yes; 0 = no)	0.466*	0.454*	0.491^{+}	0.438*	0.410***	0.387***	0.463**	0.460**	0.461**	0.463**
	(0.148)	(0.150)	(0.167)	(0.152)	(0.082)	(0.060)	(0.121)	(0.121)	(0.121)	(0.120)
Unempl. Rate (%)	0.977	0.948	0.928	0.977	0.922	0.936	0.965	0.965	0.965	0.965
	(0.118)	(0.113)	(0.101)	(0.134)	(0.067)	(0.074)	(0.099)	(0.100)	(0.100)	(0.098)
Whites 30-45, some college (1 =	1.051	0.854	1.202	1.052	1.097	1.188	1.007	1.012	1.007	0.997

yes; 0 = no)	(0.449)	(0.359)	(0.551)	(0.480)	(0.345)	(0.438)	(0.342)	(0.344)	(0.343)	(0.339)
Mexican FBNC in a state (%)	1.016	1.009	1.021	1.003	1.018*	1.026**	1.019	1.020^{+}	1.019^{+}	1.018
	(0.014)	(0.011)	(0.014)	(0.010)	(0.007)	(0.008)	(0.011)	(0.011)	(0.011)	(0.011)
Constant	1.48×10^{7}***	2.10×10^{7}***	1.64×10^{7}***	1.44×10^{7}***	1.17×10^{7}***	1.99×10^{7}***	1.92×10^{7}***	1.96×10^{7}***	1.96×10^{7}***	1.91×10^{7}***
	(9.68×10^{6})	(1.65×10^{7})	(1.30×10^{7})	(9.55×10^{6})	(1.46×10^{7})	(2.29×10^{7})	(1.13×10^{7})	(1.17×10^{6})	(1.17×10^{6})	(1.13×10^{6})
Observations	47123	44620	41938	42089	81431	64675	59279	59279	59279	59279

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 3B. Heterogeneity Effects, College Enrollment.

	(16)	(17)	(18)	(19)
	Men	Women	Married	Unmarried
FBNC after ISRT	1.151	1.404*	2.173	1.221
(1 = yes; 0 = no)	(0.312)	(0.189)	(1.137)	(0.217)
FBNC	0.956	1.163*	0.838	1.055
(1 = yes; 0 = no)	(0.172)	(0.068)	(0.284)	(0.137)
Age (years)	0.377***	0.351***	0.482***	0.363***
	(0.025)	(0.016)	(0.041)	(0.016)
Male			0.913	1.200+
(1 = yes; 0 = no)			(0.270)	(0.115)
Married	0.430***	0.476*		
(1 = yes; 0 = no)	(0.102)	(0.143)		
Unemployment Rate (%)	0.936	1.023	0.533***	0.983
	(0.065)	(0.137)	(0.073)	(0.099)
30-45 Whites w/ some college	1.14	0.816	1.135	1.016
(1 = yes; 0 = no)	(0.441)	(0.281)	(0.736)	(0.382)
Mexican FBNC in a state (%)	1.040***	1.003	1.097	1.017
	(0.011)	(0.011)	(0.085)	(0.012)
Constant	2.74×10^{7}***	3.99×10^{7}***	9.11×10^{5}***	2.81×10^{7}***
	(2.31×10^{6})	(2.80×10^{6})	(1.59×10^{6})	(1.70×10^{7})
Observations	29120	27817	6455	49056

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 4A. Sensitivity and Falsification Analyses, Associate's-Degree Attainment.

	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Traditional States	Top 10 States (1996)	Top 10 States (1996)	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	-1 Year	+1 Year	+2 Years	+3 Years
FBNC after ISRT	1.117	0.943	1.009	0.853	0.956	0.912	1.398	1.132	0.881	1.112
(1 = yes; 0 = no)	(0.325)	(0.160)	(0.279)	(0.118)	(0.143)	(0.183)	(0.488)	(0.215)	(0.157)	(0.160)
FBNC	0.178***	0.211***	0.188***	0.228***	0.235***	0.952	0.139***	0.173***	0.206***	0.178***
(1 = yes; 0 = no)	(0.049)	(0.032)	(0.053)	(0.029)	(0.024)	(0.186)	(0.049)	(0.031)	(0.034)	(0.020)
Age (years)	1.120***	1.124***	1.117***	1.120***	1.110***	1.112***	1.120***	1.120***	1.120***	1.120***
	(0.013)	(0.012)	(0.013)	(0.012)	(0.008)	(0.007)	(0.011)	(0.011)	(0.011)	(0.011)
Male	0.631**	0.612**	0.637**	0.620**	0.681***	0.696***	0.637***	0.638***	0.638***	0.638***
(1 = yes; 0 = no)	(0.068)	(0.068)	(0.076)	(0.073)	(0.040)	(0.035)	(0.058)	(0.059)	(0.059)	(0.059)
Married	0.772*	0.759*	0.757*	0.780*	0.821***	0.852**	0.798**	0.797**	0.798**	0.798**
(1 = yes; 0 = no)	(0.067)	(0.067)	(0.070)	(0.072)	(0.036)	(0.039)	(0.063)	(0.063)	(0.063)	(0.062)
Unemployment Rate	0.926	0.902	0.953	0.946	0.874***	0.888*	0.930	0.930	0.931	0.930
(%)	(0.054)	(0.062)	(0.050)	(0.049)	(0.032)	(0.043)	(0.047)	(0.047)	(0.047)	(0.046)
Whites 30-45, some college (1 = yes; 0 = no)	1.217	1.162	1.205	1.243	1.192	1.294	1.228+	1.229+	1.227+	1.228+
	(0.148)	(0.121)	(0.145)	(0.155)	(0.175)	(0.201)	(0.128)	(0.127)	(0.127)	(0.128)
Mexican FBNC	1.015	1.022	1.008	1.024	0.997	0.993	1.005	1.004	1.004	1.004

in a state (%)										
	(0.021)	(0.019)	(0.025)	(0.014)	(0.013)	(0.017)	(0.021)	(0.021)	(0.021)	(0.021)
Constant	0.009***	0.010***	0.009***	0.008***	0.017***	0.016***	0.010***	0.010***	0.010***	0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	60803	57997	53541	55521	106253	74980	78167	78167	78167	78167

$p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 4B. Heterogeneity Effects, Associate's-Degree Attainment.

	(16)	(17)	(18)	(19)
	Men	Women	Married	Unmarried
FBNC after ISRT	1.343*	0.886	0.983	1.070
(1 = yes; 0 = no)	(0.195)	(0.220)	(0.161)	(0.234)
FBNC	0.146***	0.214***	0.183***	0.189***
(1 = yes; 0 = no)	(0.024)	(0.045)	(0.026)	(0.045)
Age (years)	1.124***	1.115***	1.141***	1.109***
	(0.016)	(0.014)	(0.010)	(0.014)
Male			0.595***	0.669***
(1 = yes; 0 = no)			(0.078)	(0.049)
Married	0.746**	0.856+		
(1 = yes; 0 = no)	(0.080)	(0.066)		
Unemployment Rate (%)	0.873	0.958	0.944	0.949
	(0.151)	(0.061)	(0.159)	(0.055)
30-45 Whites w/ some college	1.555	1.107	1.751+	1.046
(1 = yes; 0 = no)	(0.501)	(0.236)	(0.492)	(0.223)
Mexican FBNC in a state (%)	1.044*	0.971	0.998	1.011
	(0.021)	(0.020)	(0.030)	(0.022)
Constant	0.008***	0.010***	0.004***	0.012***
	(0.009)	(0.003)	(0.004)	(0.003)
Observations	38138	38745	34094	42959

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 5. Sensitivity, Falsification, and Heterogeneity Analyses, Bachelor's-Degree Attainment.

	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	Traditional States	Top 10 States (1996)	Top 10 States (1996)	Early ISRT States	Non-Latino FBNC	Non-Latino Citizens	-1 Year	+1 Year	+2 Years	+3 Years
FBNC after ISRT	1.179	0.850	0.994	0.822	1.038	1.220	1.982	1.119	0.765	1.056
(1 = yes; 0 = no)	(0.627)	(0.283)	(0.493)	(0.256)	(0.274)	(0.157)	(0.901)	(0.401)	(0.209)	(0.172)
FBNC	0.161**	0.225***	0.166**	0.231***	0.203***	0.680***	0.097***	0.168***	0.215***	0.176***
(1 = yes; 0 = no)	(0.064)	(0.033)	(0.069)	(0.033)	(0.032)	(0.054)	(0.038)	(0.040)	(0.054)	(0.034)
Age (years)	1.164***	1.165***	1.168***	1.161***	1.143***	1.148***	1.161***	1.160***	1.161***	1.160***
	(0.012)	(0.011)	(0.012)	(0.013)	(0.009)	(0.007)	(0.010)	(0.011)	(0.011)	(0.010)
Male	0.613**	0.600**	0.641**	0.631**	0.674***	0.683***	0.634***	0.635***	0.636***	0.635***
(1 = yes; 0 = no)	(0.071)	(0.073)	(0.083)	(0.082)	(0.043)	(0.043)	(0.062)	(0.062)	(0.062)	(0.063)
Married	0.725*	0.718*	0.689*	0.732+	0.820**	0.832***	0.752*	0.751*	0.751*	0.752*
(1 = yes; 0 = no)	(0.100)	(0.104)	(0.100)	(0.119)	(0.053)	(0.038)	(0.091)	(0.090)	(0.091)	(0.091)
Unemployment Rate	0.994	0.961	1.002	0.960	0.898***	0.905**	0.978	0.978	0.979	0.978
(%)	(0.094)	(0.101)	(0.099)	(0.118)	(0.027)	(0.028)	(0.082)	(0.083)	(0.083)	(0.082)
Whites 30-45, some college (1 = yes; 0 = no)	0.685	0.715	0.649	0.775	1.009	1.042	0.791	0.788	0.784	0.785
	(0.239)	(0.309)	(0.233)	(0.353)	(0.249)	(0.317)	(0.226)	(0.223)	(0.221)	(0.222)
Mexican FBNC	1.000	1.006	0.994	1.012	0.956	0.959	0.990	0.989	0.988	0.989

in a state (%)	(0.028)	(0.026)	(0.033)	(0.016)	(0.026)	(0.026)	(0.027)	(0.027)	(0.028)	(0.027)
Constant	0.001***	0.002***	0.001***	0.002***	0.005***	0.004***	0.002***	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	60733	57815	53362	55351	105575	74354	77399	77399	77399	77399

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

Appendix 5B. Heterogeneity Effects, Bachelor's-Degree Attainment.

	(16)	(17)	(18)	(19)
	Men	Women	Married	Unmarried
FBNC after IIRIRA	1.345	0.937	0.834	1.341
(1 = yes; 0 = no)	(0.433)	(0.400)	(0.338)	(0.455)
FBNC	0.144***	0.195***	0.212***	0.141***
(1 = yes; 0 = no)	(0.038)	(0.047)	(0.043)	(0.042)
Age (years)	1.147***	1.171***	1.192***	1.148***
	(0.013)	(0.012)	(0.016)	(0.014)
Male			0.584**	0.671***
(1 = yes; 0 = no)			(0.106)	(0.037)
Married	0.684+	0.797*		
(1 = yes; 0 = no)	(0.132)	(0.069)		
Unemployment Rate (%)	0.928	0.986	1.073	0.948
	(0.192)	(0.033)	(0.246)	(0.048)
30-45 Whites w/ Bachelor's +	1.289	0.495*	1.716	0.607*
(1 = yes; 0 = no)	(0.563)	(0.165)	(0.627)	(0.150)
Mexican FBNC in a state (%)	1.002	0.972	1.003	0.983
	(0.024)	(0.031)	(0.035)	(0.024)
Constant	0.002***	0.001***	0.000***	0.003***
	(0.003)	(0.001)	(0.000)	(0.001)
Observations	36702	38214	33142	42047

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Robust standard errors are in parentheses. Notes: Treatment effects are from logistic regressions of the outcome dummy variable for a respondent having at least a high-school diploma. All estimates are taken from the sample of monthly Current Population Survey Merged Outgoing Rotating Group (CPS-MORG) for the years 1998-2013, and exclude observations taken before the July 1, 1998 enactment date of the Illegal Immigrant Responsibility and Immigrant Reform Act (IIRIRA). Except as otherwise noted, all observations are of respondents ages 18-21 of Mexican ancestry. Values are presented in odds-ratios.

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Certification

I, Matthew Patrick Shaw, certify that my dissertation has a total word count of 29,488 words.

/s/ Matthew Patrick Shaw